ANNUAL REVIEW OF PSYCHOLOGY

CALVIN P. STONE, *Editor*
Stanford University

DONALD W. TAYLOR, *Associate Editor*
Stanford University

VOLUME 3

1952

ANNUAL REVIEWS, INC.
STANFORD, CALIFORNIA, U.S.A.

XUM

ANNUAL REVIEWS, INC.
STANFORD, CALIFORNIA, U.S.A.

FOREIGN AGENCIES

H. K. Lewis & Company, Limited
136 Gower Street,
London, W.C.1

Mezhdunarodnaya Kniga
Kuznetsky Most, 18
Moscow

Martinus Nijhoff
9, Lange Voorhout
The Hague

Maruzen Company, Limited
6, Tori-Nichome Nihonbashi
Tokyo

PRINTED AND BOUND IN THE UNITED STATES OF AMERICA BY
GEORGE BANTA PUBLISHING COMPANY

XUM

BF
1
A77
(1950)

835415

PREFACE

Taking stock of essentially new and important contributions to psychology from all laboratories of the world is a major objective of the *Annual Review of Psychology.*

Our inability to schedule certain foreign scholars, preoccupied as they were with heavy teaching and administrative responsibilities following the war, caused Volumes 1 and 2 to fall short of this objective. Therefore, it was a happy occasion for the Editorial Committee when it could announce that three European scholars had agreed to prepare reviews for Volume 3. They were Drs. J. Elmgren of the University of Gothenburg, H. J. Eysenck of the Maudsley Hospital and University of London, and J. Piaget of l'Institut de Rousseau.

With regret, Dr. Piaget, late in February, 1951, asked to be released from his review of child and adolescent psychology because of uncertain health and arduous responsibilities connected with his work with UNESCO. Fortunately, Drs. Vincent and Helen H. Nowlis agreed to fill the breach occasioned by Dr. Piaget's resignation and, despite the relatively short time remaining and other commitments which could not be put aside, provided us with an excellent chapter on Child Psychology.

In Volume 3 will be found some evidence of a greater breadth of selection of papers from outside the United States than heretofore; particularly is this true of the chapters on Somesthesis and the Chemical Senses, on Personality, and on Comparative and Physiological Psychology. This development we hope marks the beginning of a trend that will increase in future volumes. For Volumes 4 and 5 the Editorial Committee has already secured or is in correspondence with additional foreign scholars for reviews in their respective fields of specialization. The emphasis on balance between domestic and foreign authors stems from the belief that certain advantages, unattainable at present by American psychologists, are readily gained by authors who have that on-the-ground, neighborly acquaintance with research and scholarly writings of continental scholars whose publication media are the French, German, Italian, Scandinavian, and Slavic languages, rather than English. Preparation of reviews by scholars from diverse countries should accelerate the gradual emancipation of psychology as a science and profession from national boundaries, and thereby provide a powerful instrument for constructive integration of the advances in psychology whatever their points of origin.

Conversations and occasional letters addressed to members of the Editorial Committee have indicated that a good many readers of Volumes 1 and 2 have hopefully awaited the time when the bibliographies might contain full titles of all literature reviewed. Being made aware of this desire, the Board of Directors of Annual Reviews, Inc., has reviewed its policy with respect to citations and agreed, on a trial basis, to permit the inclusion of

XUM

full titles of articles, monographs, and books, in Volume 4 (1953), by the authors of individual chapters who desire to do so. Already several reviewers have indicated their intention to make complete citations. Their chapters should provide a basis for deciding whether the additional space required by full titles, which is in competition with space for exposition, should be used for titles in subsequent volumes. Opinions on this matter from readers are invited by members of the Editorial Committee.

J.E.A. C.I.H.
J.G.D. J.G.M.
C.H.G. D.W.T.
C.P.S.

XUM

TOPICS AND AUTHORS

ANNUAL REVIEW OF PSYCHOLOGY

VOLUME 4 (1953)

CHILD PSYCHOLOGY, *D. B. Harris*

LEARNING, *B. J. Underwood*

VISION, *M. D. Vernon*

AUDITION, *J. C. R. Licklider*

SOMESTHESIS AND THE CHEMICAL SENSES, *T. C. Ruch*

INDIVIDUAL DIFFERENCES, *A. Anastasi*

PERSONALITY, *U. Bronfenbrenner*

SOCIAL AND GROUP PROCESSES, *T. M. Newcomb*

INDUSTRIAL PSYCHOLOGY, *T. W. Harrell*

COMPARATIVE PSYCHOLOGY, *E. H. Hess*

PHYSIOLOGICAL PSYCHOLOGY, *W. D. Neff*

ABNORMALITIES OF BEHAVIOR, *R. W. White*

CLINICAL METHODS: PSYCHODIAGNOSTICS, *J. B. Rotter*

CLINICAL METHODS: PSYCHOTHERAPY, *R. N. Sanford*

EDUCATIONAL PSYCHOLOGY, *H. D. Carter*

COUNSELING METHODS, *E. G. Williamson*

STATISTICAL THEORY AND RESEARCH DESIGN, *F. Mosteller*

SPECIAL DISABILITIES, *K. Cobb*

PSYCHOLOGICAL THEORY, *G. Bergmann*

XUM

XUM

CONTENTS

	PAGE
CHILD PSYCHOLOGY, *V. Nowlis and H. H. Nowlis*	1
LEARNING, *H. F. Harlow*	29
VISION, *H. Helson*	55
HEARING, *W. R. Garner*	85
SOMESTHESIS AND THE CHEMICAL SENSES, *G. R. Wendt*	105
INDIVIDUAL DIFFERENCES, *L. G. Humphreys*	131
PERSONALITY, *H. J. Eysenck*	151
SOCIAL PSYCHOLOGY AND GROUP PROCESSES, *M. B. Smith*	175
INDUSTRIAL PSYCHOLOGY, *C. W. Brown and E. E. Ghiselli*	205
COMPARATIVE AND PHYSIOLOGICAL PSYCHOLOGY, *H. W. Nissen and J. Semmes*	233
ABNORMALITIES OF BEHAVIOR, *J. Zubin*	261
CLINICAL METHODS: PSYCHODIAGNOSTICS, *A. Magaret*	283
CLINICAL METHODS: PSYCHOTHERAPY, *V. C. Raimy*	321
COUNSELING: THERAPY AND DIAGNOSIS, *W. M. Gilbert*	351
EDUCATIONAL PSYCHOLOGY, *J. K. G. Elmgren*	381
STATISTICAL THEORY AND RESEARCH DESIGN, *Q. McNemar*	409
MOTIVATION, *O. H. Mowrer*	419
INDEXES	439

Annual Reviews, Inc., and the Editors of its publications assume no responsibility for the statements expressed by the contributors to this *Review*.

XUM

CHILD PSYCHOLOGY[1]

By Vincent Nowlis and Helen H. Nowlis
Iowa Child Welfare Research Station, State University of Iowa, Iowa City, Iowa

Relationships of Age and Sex with Behavior

As usual, a large proportion of the research studies in child psychology are concerned with the relation of age to various behavioral characteristics. We review here nineteen of these studies, involving a total of only 9,000 infants and children. The expressed aim of many of these investigators is the establishment of norms for use in the clinical appraisal of the child.

Motor responses.—Gesell & Ames (37), in one of three studies reported from the Gesell group this year, examined postural behavior patterns in infants of fetal age 28 to 40 weeks and in 15 postfetal age levels from 4 to 56 weeks. Data concerning the tonic-neck-reflex (asymmetric) pattern and the symmetrotonic pattern suggest that the shift from dominance of the former to that of the latter exemplifies such principles as developmental direction, functional asymmetry, and reciprocal interweaving. Spitz (112) describes a reflex in the newborn infant that he had not found previously described in the literature. Gentle stroking of the dorsal surface of the end phalanxes of the four fingers simultaneously elicits extension of one finger, (usually fifth or index), and then the other fingers, sometimes involving also thumb abduction and extension and wrist flexion. In the newborn, this reflex could be elicited more reliably than any other, with the exception of the grasping reflex. It disappears in the course of the first three months. He names it the "digital extension reflex."

Speech.—Irwin (56) continues his reports of infant speech development with a presentation of curves regarding frequency of classes of phoneme types which indicate that the course of development of initial consonants during infancy is linear, that of medials, decelerating , that of finals, accelerating. Metraux (74), using phonetic speech transcripts obtained from 207 children in the Gesell clinic and nursery school, presents a composite picture of speech at seven age-levels from 18 to 54 months. Trends are described in such areas as pronunciation, voice, repetitions, and language relations to others. An exploratory study of the concept of idiopathic language retardation by Goldenberg (40) illustrates some of the difficulties encountered in the clinical use of norms. Blomquist (16) uses 60 children, ages 9, 10, and 11, to investigate the relation of age to maximum rate of repetition of the sounds: p, t, k, and the serial combination, ptk. The older children had faster diadochokinetic (repetitive) movements.

[1] The survey of the literature to which this review pertains was completed in May, 1951.

Copying and reading.—A contribution to the ongoing enterprise of developing norms for the assessment of academically relevant skills in the school-age child is made by Townsend (125) in a study of copying in 287 school children aged six to nine years. Copying as measured by use of nine Bender-Gestalt figures and eight simpler figures correlated more highly with form perception (r of .60) than with motor ability and showed more obvious improvement with mental age than with chronological age. The roots of reading ability are taken back to age 15 months in a study by Ilg & Ames (55) of 30 to 50 or more children at each of various intervals from 15 and 18 months to nine years. The responses of the younger children to books and pictures (pats or points to identified picture, etc.) were observed in naturalistic settings; older children were given Gray's Reading Readiness and Reading Paragraphs tests. Particular emphasis is placed on type of error as hint for developmental diagnosis. The authors helpfully discuss the relationship of their developmental viewpoint, as exhibited in a reading gradient of profiles for each of the above stages, to educational practice. It does not seem to us either fitting or helpful, however, to claim that "these and other stages follow each other in lawful, inevitable order" when the evidence is so inadequate. Developmental principles inferred from simple behavior in young subjects can and should suggest where to look for functional relationships in more complex behavior, like reading, but the generality of laws depends on fact rather than fiat.

Concepts.—Piaget, as source of ideas in this area, was repeatedly acknowledged this year. Dennis (25) gave a strongly favorable evaluation of Piaget's contributions, placing in perspective his own original outrage over Piaget's weaknesses. Strauss (121) re-examined the 1945 Huang-Lee data (54); he found they are not a decisive test of the theory of egocentric animism, and rejected their rejection of the Piaget hypothesis. With Schuessler, Strauss (102) ingeniously used scale analysis in the tentative establishment of sequential stages in the understanding of the concept of money among 68 middle-class children, ages 4 to 10. Ordering of the data with the scalogram helped the authors to detect interesting progressions in certain aspects of behavior with money, as from concrete to abstract, rigid to flexible, and absolute or egocentric to relative. The important general goal of these authors is "to state with some assurance how a child's responses must be organized in order that he learn to act in a still more complicated fashion." Should they, in promised future research, begin to consider the problem of controlling and producing such learning, we may expect to find a more adequate description of response organization than that provided by scale analysis alone.

Closer scrutiny of the process of learning of concepts was achieved by Werner & Kaplan (130), who gave 125 children, $8\frac{1}{2}$ to $13\frac{1}{2}$ years of age, the task of finding the meaning of 12 artificial words, each of which appeared in a different set of six sentences. The subject was repeatedly asked how and

XUM

why the meaning he gave for the word in each of the six sentences fitted into that sentence and the preceding one. The protocols suggest that word meanings are acquired through a large variety of processes. Younger children often let the word carry the meaning of the whole sentence or context in which it occurred. Older children often used forms of signification which were not circumscribed or stable; Werner calls these holophrastic concepts. Here, as in the previously reported study, we wonder whether dependence on age or developmental level as a primary variable may have prevented the investigators from searching for relationships between concept achievement and more important antecedent conditions. Age is too easy; it is a weak partner to these other measures, obtained with such ingenuity and understanding.

A similar study by Feifel & Lorge (33) seems more definitive and certainly easier to evaluate. The verbatim definitions of 900 children, ages 6 to 14, to the Stanford-Binet Vocabulary Test were classified according to five categories developed by Feifel in an earlier study. Older children significantly more often use synonyms and explanations. As with other studies, these authors emphasize that the method of approach used by an individual is as revealing of his developmental status as is his passing or failing the word. Behavioral variables like those developed in this and the two preceding studies, together with the new concepts and hunches developed by Dollard & Miller (28), in their analysis of thinking and its interrelations with anxiety, should encourage a stronger experimental attack on concept formation. It occurs to us that we know of no carefully reasoned justification for the use of children in preference to adults as subjects in such experimentation, over and beyond the one goal of ontogenetic analysis. Surely there is one.

Preferences, evaluations, and attitudes.—From Syracuse came two studies involving age-trends. Together, they supply about one-half of all the subjects involved in the normative studies reviewed here. One study by Butterworth & Thompson (19) will be discussed under mass communication; the other, by Witryol (133), will be discussed under the section on the school as social environment. Several other studies relating age to social behavior (22, 23, 49, 60, 113) are also reviewed below.

Miscellaneous developmental studies.—Banham (8) supplements her well-known 1932 (K. Banham Bridges) analysis of emotional development in early infancy with a report on the development of affectionate behavior, based on incidental observations of over 900 infants during the course of psychological examination of mental level. Thetford, Molish & Beck (123), using 155 normal children, aged 6 to 18, to establish Rorschach norms for the assessment of deviants found that they had acquired knowledge of normal development which had intrinsic value. They report and document the changes observed through Rorschach analyses in the areas of intellectual activity (greater recruitment of energy in meeting demands of environment,

XUM

increased ability in meaningful organization of perception, increase in conformity and in communication of popular terms), effective reactions, and fantasy life. Rosenzweig & Mirmow (100), as part of an attempt to validate the theory that "trends" in the Picture-Frustration study imply instability of response to frustration and low frustration tolerance, analyze the records of 272 children, ages 4 to 13. An illustration of trend is given in the case of a subject who, through guilt over his earlier overt expression of hostility, shows in the latter half of his record a significant shift from extrapunitiveness toward intropunitiveness. Four and five-year-olds showed significantly fewer trends than older children, a finding the authors very tentatively attribute to the consequences of socialization in older children, namely, the inhibition of agression and rise in conflict.

In so far as the studies reviewed in this section were designed as normative studies, they must in general be severely criticized for dependence on very small numbers of subjects. In so far as they have simply attempted to demonstrate that age is related to certain measures of behavior, they add to the evidence which has been accumulating for many years. Conspicuous by their rarity are studies which attempt to find the joint influence of age and other factors. Also notably rare are studies which attempt to conceptualize the age factor in terms of culturally recognized age grades; this trend is more pronounced in studies of sex differences, an area wherein most investigators are now aware of the possible influence of sex-typing. It is, thus, a pleasure to encounter the Jones & Bayley (61) study of physical maturing among boys as related to behavior. As a measure of physical maturation during adolescence, they use skeletal age, which is correlated with such external signs as size, strength, and certain primary and secondary sexual characteristics. By separate study of the 16 latest maturing and 16 earliest maturing in a group of 90 boys, they find evidence that the external signs of maturation elicit differential social responses from peers and adults. At the same chronological age, the physically accelerated are more likely to be accepted and treated as mature boys, the retarded are expected to act like little boys, and tend to do so. The former seem more secure in their social status, better groomed, and more relaxed and unaffected; the latter are rated as more eager, uninhibited, and animated. Nevertheless, these differences are not reflected in such traits as popularity, leadership, or prestige, a finding which the authors tentatively attribute to the activity and other compensations of the late-maturers and to the handicaps which sometimes befall the early-maturer, such as severe acne or outgrowing his own age group.

Sex.—In another University of California study, by Honzik (53), sex differences in the extent to which preadolescents included a variety of play materials in setting up an imaginary movie scene were observed and a Masculinity-Femininity scale constructed on the basis of these observations. Ratings of somatic androgyny were not generally related to the Masculinity-Femininity scores nor was reputation with classmates. The author concludes

XUM

that the sex differences displayed in this particular situation are consonant with the roles boys and girls are expected to play in our culture and have little direct relation to biological differences; nevertheless, the greater activity of boys at all ages and in all pursuits and their greater range of behavior are laid to physical rather than to cultural differences.

A study by Rabban (93) seeks to determine whether sex role is determined by social class membership as well as by general cultural factors. Fifteen boys and 15 girls from each of five age groups (3 to 8 years) were chosen in each of two communities—one middle class and one working class community. The children were required to choose among 16 toys, eight of which had been selected as suitable for boys and eight suitable for girls, to indicate among six dolls the one most like himself, and to identify the sex of each doll. These data were supplemented by observations, by interview with parents and teachers, and by case studies of four children from each social group. Results indicate that boys are more clearly aware of sex-appropriate behavior than are girls and that both boys and girls of the working class are earlier aware of the sex-role pattern than those of the middle-class group. Weider & Noller (129), studying the sex awareness of eight to ten-year-olds by means of drawings of the human figure, found that socio-economic status was related to the number of characteristics drawn for both male and female figures in both boys and girls, but that "identification," as revealed by which sex was drawn first and larger, was unrelated to socio-economic status. There was a tendency for girls to identify more with their own sex than for boys. The age difference of the subjects together with the different indices of sex awareness may well account for the inconsistencies between these results and those of the previous study.

Bayley (11) compared the somatic androgyny ratings and Kuder Interest Records of 162 normal adolescent boys and girls. Androgynic variables in body form were found to be significantly related to strength and in some degree to weight; muscles and strength tend to go with greater masculinity; fat and a "delicate" skeletal frame go with greater femininity. Intelligence showed no relation to sex variables, either physical or behavioral. Both boys and girls whose builds are not completely sex-appropriate tend to have interest scores which deviate in the masculine direction from the average interests of their own sex. The author concludes that somatic androgyny is only one of a multiplicity of factors which may influence the direction of a person's interest.

Pointing up the importance of the child's response to physical sex characteristics is the study of Schonfeld (101), based on clinical observation of 256 boys ranging in age from 9 to 16 years over periods ranging from six months to over 10 years. Physical masculinity is defined in terms of genital maturation associated with rapid acceleration of growth in height, body breadth, weight, size of heart, capacity of lungs, and muscle strength. Clinical observations lead the author to the following conclusions: the onset of

XUM

adult sexual interests and the emotional phenomena associated with pubescence seem to be related to psychosexual development and the social pressures of cultural patterns rather than to hormone stimulation; parents, teachers, and companions play a decisive role in formulating standards of masculinity; and discrepancies between the physical signs of maturity and the group concept of normal are made clear to the boy. It is his reaction to being considered different rather than delayed or deficient masculinity itself which affects the personality and behavior of the boy. While physical treatment such as hormone induction of pubescence or use of plastic surgery is to be regarded as a necessary part of treatment, the proper application of psychotherapy is the mainstay of treatment. Abel & Joffe (1) provide a well-documented study of the beliefs and practices, in regard to female puberty, particularly menarche, of German, Polish, Irish, Orthodox Jewish, and American groups in America; on the basis of data secured through interviewing, they also show how these different attitudes become organized in the second- or third-generation adolescent girl.

Interrelationships of Behavior With Handicap and Illness

Numerous studies have appeared which report the effects of a wide variety of handicapping conditions on a wide variety of aspects of behavior. For the most part, these fall more properly in the areas of individual differences or abnormalities of behavior, despite the fact that many of them use children as subjects or deal with conditions prevailing in early years. There are, however, a few noted here which are primarily concerned with personality dynamics as etiology in psychosomatic conditions or as consequence of handicapping conditions.

Miller & Baruch (75, 76) report two studies of a group of 90 allergic children (primarily asthma and hayfever), the first of which analyzes emotional traumata preceding the onset of allergy and the second compares manifestations of hostility in allergic and nonallergic child clinic cases. Information on traumatic episodes apparently related to the first onset of symptoms was found in 68 of the 90 allergic cases and loss or threatened loss of a parent was prominent in the largest number of these episodes. All of them were related to fear of rejection by the mother, and the episodes served to confirm this fear. The allergic symptoms are considered manifestations of blocked resentment toward the mother. Maternal rejection and blocked resentment of the mother also play a large part in the manifestations of hostility in allergic children, hostility being the concern of the second of these studies.

Maternal rejection was present in 98 per cent of the 90 allergic subjects, while in the nonallergic group ($N = 53$), it was present in only 24 per cent. Blocking of outgoing hostility was present in 92 per cent of the allergic children while none of the nonallergic children who were maternally rejected showed this blocking of the expression of hostility. The allergic symptoms are interpreted as means of releasing the tension represented by insoluble

XUM

conflict and blocking. Asthmatic children in the study of Harris, Rapoport, Rynerson & Samter (48) were characterized by fear of separation from the mother figure; they had greater difficulty in crying and in confiding in the mother in contrast to a group of children with rhinitis and a group with nonallergic behavior problems. Evidence of lack of identification of the mother with the child was prominent in both the asthmatic group and the behavior problem group, but was low in the rhinitis group. One would wish for more rigorous definitions of the behavior variables used, but the apparent magnitude of the differences found strongly suggest the importance of certain aspects of the mother-child relationship in the etiology of asthma.

In the studies of the effects of handicapping conditions on personality and adjustment, the reactions of the handicapped, his family, and his peers to the condition seems more crucial than the actual physical or mental handicap. Dubo (30) in a psychiatric study of children with pulmonary tuberculosis found reaction to the illness characterized by marked overwhelming anxiety, morbid preoccupations, fears of death, attempts at denial and projection, and severe identification problems with striving to identify with the normal. Harris (45) found that, apart from simple strength and endurance factors, the only behavior or personality trait on which teachers were able to distinguish 43 postpolio cases in the elementary schools was self-discipline, and he suggests that this is related to indulgence of a presumably weakened child rather than to any direct effect of the disease. Results of a study of 30 crippled children by Broida, Izard & Cruickshank (18) led the authors to hypothesize that those crippled children whose adjustment is impeded by intense feelings of fear have a need for greater social acceptance by and social activity with their contemporaries. Lack of such acceptance and activity and predominance of guilt thema in two groups in which the need to be free from intense feelings of fear was being either under- or over-met is suggested as reflecting lack of social judgment, social experience, and social maturity. Fitzgerald (34), in a study of success-failure and Thematic Apperception Test reactions of orthopedically handicapped children, tentatively concludes that family interactions, parent and sibling practices, and attitudes as seen by the subject, are a source of relatively greater frustrations and tensions than are physical handicaps as such. Even the handicap of mental defectiveness is complicated by the reactions of others to the defect and of the defective to these reactions. Beier, Gorlow & Stacey (12) found a higher percentage of themes of negative emotion, anxiety, separation, and rejection in a group of 40 institutionalized adolescent girls than Symonds (122) had reported for a normal group. Although there were also fewer themes of agression, the aggression represented was more brutal and direct. Johnson (59), comparing the social position of mentally handicapped in regular school classes with that of typical children, found that they were less accepted and more rejected and that rejection decreased as mean intelligence scores for the mentally handicapped increased. The reasons given for rejection were not low mental abil-

ity, but unacceptable social behavior and apparent inability or desire to conform to group standards of behavior. A significant and different approach to the relation of handicap to behavior is found in the study of Jervis & Haslerud (58), who report on the differences in response to frustration of blind and sighted adolescents. Twenty blind, twenty sighted blindfolded, and twenty sighted, not blindfolded, adolescents were tested with six manual puzzles, the last three of which were not only unsolvable but also accompanied by goading to produce frustration. Verbal responses were recorded and overt physiological behavior noted. Quantitatively, the blind significantly exceed the sighted in both verbal and overt physiological responses. Qualitatively, the blind show significantly more physiological responses involving auditory clues (sighing, uneven respiration) and fewer responses involving visual clues (flushing, biting tongue and lips). The blindfolded correspond to the sighted in this respect. Intropunitive scores for the blind were 58 per cent in contrast with 28 and 30 per cent for sighted groups.

Experimental Modification of Behavior

As in the preceding sections, we find here that very few workers seem to be investigating interrelated problems. There is not much in common among the goals and the methods of the disappointingly small number of experimentalists who have published papers this year. Alberts & Ehrenfreund (4) report on a sequel to the 1946 Kuenne (64) study of transposition. Whereas Kuenne tested at only two points, one close to and one distant from the original pair of stimuli, the present workers investigated these two and also three intermediate points, finding, as expected from the former study, that amount of transposition falls sharply in three-year-olds with increasing distance from the original pair while the gradient is almost horizontal for $4\frac{1}{2}$ to $5\frac{1}{2}$-year-olds. They attribute this difference to the operation of verbal control in the older children; unfortunately for Kuenne, knowledge about response-produced cues was even less well developed at the time of her experiment than it is now, and her analysis of the relation between verbalization and transposition was very incomplete. It is surprising, however, to find that Alberts & Ehrenfreund pay even less attention to this important problem than did Kuenne. They do, however, definitely advance beyond Kuenne in bringing about much more rapid learning; whereas the earlier study ran to the discouraging total of 7,000 training trials for 44 subjects (on a very simple and obvious size discrimination) the present sudy required a more reasonable total of 1,600 trials for 40 subjects to reach a similar criterion. Improvements in apparatus and procedure were these: the children were permitted to open the wrong box when choosing incorrectly; the wrong box was black inside while the correct one was white, thus reducing transfer of secondary reinforcement; the apparatus forced more direct contact with the size characteristics of the stimuli; the children received gumdrops rather than temporary possession of colored toys, as in Kuenne's study. We wish also to suggest still another

XUM

improvement for such studies, one which eliminates the necessity of rewarding during test series: with minor preparation, young children will readily work through a short series of test trials for an accumulated reward deferred to the last trial. We should also like to see a frontal attack made on the general problem of incentives in studies of this kind with children. Were the presently used gumdrops more effective than Kuenne's toys; if so, how? Our systematic knowledge about the effects of degree of reinforcement on learning is not yet self-consistent; but on the practical side, experimentalists with children know that type and context of incentive are related to the child's continued interest in the learning task. Countless studies lie ahead in which the behavior of children will be modified through the operation of reinforcement. The probable importance of such laboratory learning experiments in the development of child psychology cannot be foretold, but we are confident that their importance will increase as we learn to be more skillful in eliciting complex behavioral systems (24, 52). Such work will require dependable knowledge about the relative value of various practical and innocuous incentives within certain specified contexts. The co-operation of experimenters, teachers, and parents is required; given a chance, the children are the most enthusiastic participants of all.

Whereas the above study contributes in a small way to a growing body of systematic knowledge about stimulus-response laws, the following two experiments are not related to systematic theory. To test the more general hypothesis that the young child, in drawing a man, emphasizes parts that are of special concern to him, Harris (46) developed several more specific hypotheses and tested them by having four groups of first grade children make a drawing of a man on 10 successive school mornings; one group had a 15-min. period of choreographic exercises emphasizing arm and leg movements before each drawing session, a second group had no such exercises, the third and fourth groups had five days with and without exercises. The hypothesis that kinesthetic experience would not be projected into subsequent drawings failed of rejection with respect to all the criteria (length of arm, of legs, etc.), but Harris did find marked consistency through the 10 days both of IQ as estimated by the drawings and of particular style. Experimental use by Keister (63) of another school resource, the mid-morning feeding of juice, did produce changes in the behavior of preschool children. Combining measures of nervous habits, hyperactivity, withdrawing and hostile behavior into one general category called negative behavior, Keister found that children given 4 oz. or more of fruit juice at 10 a.m. each morning for 10 weeks showed significantly less negative behavior during the remainder of each morning than did children given water. The difference due to juice was particularly apparent in the two-year-old children.

Benjamins (14) reports an interesting attempt to modify experimentally the self-concept of 48 high school students and to determine the relation of such change to changes in behavior on an intelligence test. The study derives

from the important concerted attack on self-concept theory at Ohio State University, but unfortunately the report is difficult to evaluate because of inadequacies of reporting. In brief, the author was able to predict fairly well whether or not performance on an intelligence test would improve on the basis of an analysis of the subjects' responses to false reports of their standing on a previous intelligence test. However, no distribution of changes in IQ is presented, nor is there any careful or helpful description of the manner in which the responses to false report were combined into the categories which permitted successful prediction. Stone & Lynn (120) also studied the influence of false-report, by reporting in a group camp situation to thirty 9 to 11-year-old boys knowledge of their results in mirror-drawing performance. False-success and false-failure did not provide as rapid learning as did report of actual performance.

Like Benjamins, Sherman & Bell (107) use the high school class room as locus for experiment. One hundred fifty-nine high school juniors were measured for memory-recognition of words which had been learned incidentally; 87 were then given an extremely difficult test of arithmetic and general intelligence (frustration experience), while the others were given easily comprehended material (success); they were all then retested for recognition of the words. The mean increase in error scores was not significantly greater after frustration than after success, but the variance of the error change scores was significantly smaller after frustration, while the correlation between initial and final scores was higher. Ratings by teachers regarding ease with which the student was readily disturbed by frustration were associated with relatively greater increase in errors made after experimental frustration.

Stiles (117) designed for fourth, fifth, and sixth grade children a learning program which presented material containing presumed relationships between such factors as motives, frustrations, and inadequate knowledge to such behavior patterns as teasing and bullying. She attempted to measure the effects of such a program on overt social behavior in the following way: students in each of six class groups wrote anonymously the reasons for his vote on certain conduct issues which were discussed in the regular weekly room council; these reasons were collected during the four councils preceding the six day learning session and during the subsequent four councils. Each of these reasons was rated on a seven-point scale according to the degree to which it showed recognition of causes which the author presumed to be involved in the conduct problem under discussion. A small but statistically significant improvement was found. The author avoids the problem of the relation of such verbal justifications to other easily obtainable overt behavior, such as the type of vote cast, or social interaction with peers during the room council. This leaves us with only a demonstration of the modifiability of verbal behavior, but the author is to be commended for not claiming more.

Berenda (15) has succeeded in producing much more definite changes in verbal behavior. In four experiments, individual children or minority groups

XUM

of children reported their judgments of the length of lines. In one experiment, individual children gave markedly wrong estimates which tended to agree with the deliberately erroneous estimates of eight enthusiastic "stooges." A second experiment showed that the teacher had no such effect on a single child; social pressure, even from the group, was also ineffective when the task involved an easy and definite judgment of relationship. A fourth experiment found that a minority of eight in the back of the room followed the deliberately erroneous majority. Through interview, Berenda secured some of the factors which preceded and accompanied the child's decision to follow the group. Younger children were more suggestible than older.

Ausubel (7) has developed a measure of prestige motivation which should prove useful to some of the increasing number of experimenters who use the class room as source of subjects and locus of experiment.

Relationships of Sociocultural Factors to Behavior

Ethnological groups.—There is said to be much research activity at the present time by social anthropologists regarding child-training practices and their consequences, but we have not been very skillful in finding relevant publications. Erikson's fine book (31) contains data which were collected years ago among Sioux and Yurok children and have been previously reported. Paul (85) gives a good short account of the socialization process in a Guatemalan village and focuses attention on a particular ritual which probably expresses recognition of sibling rivalry. Devereux continues his abundant reports on the Mohave Indian with no less than seven articles containing information about child behavior. His study of status, socialization, and interpersonal relations is typical (27). Gorer & Rickman (43) relate the national character of the people of Great Russia to child training practices, with special emphasis on the hypothesized consequences of swaddling. Barnouw (10) applies the cultural and personality approach to the history of the Wisconsin Chippewa, attributing their ready compliance to white domination and their failure to develop contemporary co-operative ventures to certain child practices, such as disciplinary techniques involving much scaring, the mutual isolation of small family groups, the relative overimportance of the father, and great pressure toward independence together with early punishment of dependency. In a carefully documented and nicely designed study of social control in Paiute, Whiting (131) supplies data secured by several methods on the control of aggression in the family. Here childhood aggression is strictly controlled by direct punishment and by the example of the parents who show aggression-anxiety and guilt even over mean thoughts and bad dreams. Self-control of aggression is then maintained through adulthood, with occasional outbursts either toward the spouse, who is the least essential person in the Paiute family, or toward the self.

Murdock and others working with the Human Relations Area Files have published an *Outline of Cultural Materials* (80) to facilitate the use of their

extensive cross-cultural materials and also as an aid to field work. Categories 85 and 86 are concerned with infancy, childhood, and socialization.

Class, regional, racial, ethnic, and status factors.—The search for correlates of social class factors yielded several interesting studies. Phillips (86) developed for use with high school students a social class questionnaire consisting of three scales, one for each of the three main social classes. Appropriate reliability and validity studies indicate that the social class membership of the student can be identified on the basis of his expressed agreements and disagreements of these three scales. Other results showed that score on the lower class membership scale is associated with lower scores on intelligence and personality tests and that the boys' scores were higher on the lower class scale while the girls' scores were higher on the middle and upper class scales; Phillips attributes this in part to the greater interest of the girls in morals, etiquette, and conventional behavior in contrast to the greater aggressiveness of the boys. Centers (22) found that 94 per cent of the students in a New Jersey high school identified themselves with one of these four classes: upper, middle, lower, or working class. The occupation of the parent was strikingly associated with the class identification expressed by the child; nevertheless, in comparing these results with the data secured from a nationwide sample of adults, Centers found that the adolescents tend to identify with the upper and middle classes more frequently than do adults. Moreover, class identification with the upper and middle classes decreases with age. Girls identify higher than do boys; Centers tentatively attributes this to the boys' greater concern about and orientation toward their forthcoming occupational status.

Whereas Mummery (79), in a careful repetition of her Iowa ascendant behavior situation, found that there were no significant differences between the Southern and Northern nursery school children tested, McCary (68) reports that the Rosenzweig Picture-Frustration Studies yield interesting regional, racial, age, and sex differences between Southern and Northern students; in general, the latter were more extrapunitive, the former, more intropunitive and impunitive.

Problems involving racial and ethnic factors have occupied a relatively large group of investigators in child psychology this year. Skillful new methods for securing data from children have been developed while other studies use more traditional attitude scales in discovering new relationships. An entire number of the *Journal of Negro Education* (62), with 22 contributions, is devoted to research and thinking about Negro children in the American social order.

Ammons (6) has adapted doll play methods to a more or less standardized interview in which young children are asked a series of questions about skin color and facial features during the course of more spontaneous doll play. Refusals to answer specific questions were not frequent. Radke & Trager (95), in an ingenious attempt to elicit descriptions of social roles of Negroes and

XUM

whites, developed formboards in which adult white and Negro men and women could be differentially clothed, placed in houses, and given different work to do. The interpretations of the various assignments provided abundant data regarding young children's social perceptions of race. The general results concur with the conclusions of the authors in an earlier publication (96). Radke, Sutherland & Rosenberg (94) also developed a picture test for use with groups of young elementary school children. They selected 32 photographs of Negro and white children, equating them on the basis of desirability or pleasantness of appearance. Four slides, each with eight portraits, were projected to the class, which was asked to write individual answers to such questions as: "Which one of these children is the smartest in school?" Sociometric data on the pictures and on actual friendships were also obtained. The test was developed for evaluating the effectiveness of a community project in changing the attitudes of Negro and white children toward each other and was given in a city school in which only 10 to 15 per cent of the students were white. These white children preferred other white children as friends, and assigned undesirable personality characteristics to the pictures of Negro children. In contrast, the Negro children showed a much less positive attitude toward their own race.

Johnson (60) has developed an interesting projective picture test in which six line drawings showing dyadic social situations are presented to children. Each picture is presented more than once, with various changes in the social identification of the two characters. Verbatim protocols of the stories told by the subjects are taken, and a prejudice score is obtained from the themes of the story. When the test was given to Spanish and Anglo[2] children in the Southwest, different sequences in the development of prejudice were found: the four-year-old Anglos were the more prejudiced despite less experience with Spanish; the Spanish rapidly developed prejudice between the ages four and eight; and the Anglos had the greatest increase between eight and twelve, when they became the most prejudiced of all.

Springer (113) carefully selected thirty 5 in. $\times$ 7 in. colored photographic portraits from a pool of 3,000 proofs in a studio specializing in children's photographs to represent boys and girls in the various national-racial groups of Hawaii. These were shown to 287 preschool children of varied national-racial background as a test of their ability to identify themselves and their siblings. Accuracy of self-identification and sibling-identification was approximately equally high at all ages; reasons given for identification mentioned hair and eye characteristics most frequently. Children of all ages tended to prefer the portraits of their own race, with the tendency strongest for the Caucasians.

Clark & Clark (23) now add a coloring test to their two other well-known tests (line drawings and dolls) of racial identification and preference in Negro

[2] Non-Spanish Caucasian.

children. The child is given a box of various colored crayons together with a sheet of outline drawings and asked to color a boy the color he himself is, and then to color a girl the color he likes little girls to be. The superiority of this test over the other two lies in its supplying a greater range of responses. Among 160 children, those who colored the outline of the child with a color reasonably close to their own for the identification response comprised 80 per cent of the five-year-olds, 86 per cent of the six-year-olds, and 97 per cent of the seven-year-olds. These colors did tend to be somewhat lighter than their own, and some children used markedly lighter colors or bizarre colors. However, only 37 per cent of the five-year-olds, 40 per cent of the six-year-olds, and 65 per cent of the seven-year-olds used brown for the preference drawing. More articulate expression of the emotional problems of young Negroes is reported in a study by Goff (39), who interviewed 150 Negro boys and girls aged 10 to 12 in New York City and Saint Louis. These informants told of 478 difficulties encountered because of their group identity, which the author classifies as follows: direct ridicule, physical ill-treatment, rude treatment, discrimination, and indirect disparagement.

In a very free and unguided interview lasting 1 to 1¾ hr., James & Tenen (57) had each of 120 English schoolgirls, ages 11 to 15 years, talk freely about what they knew of certain peoples of the world. Data are presented to show the various preferences of the subjects, the labels they attached to various groups, and the circumstances under which they changed their attitudes toward a certain group. Zeligs (135, 136) continues her reports on data secured with an attitude test regarding various ethnic groups, together with the reasons children give for their preferences and rejections.

Gough, Harris, Martin & Edwards (44, 47) explored the relationships of children's ethnic attitudes to other personality factors and to parental beliefs regarding socialization practices. An attitude scale toward Negroes was given to students in two schools probably differing in mean level of social class, together with other scales involving personality inventory items, social distance items for Jews and Negroes, and items related to general intolerance. The children showing greatest intolerance toward Negroes and Jews also tended to be more constricted, cynical, and fearful, less confident and secure, and more suspicious and ethnocentric. Questionnaires returned by 154 of the mothers of these children provided data regarding their beliefs about child-rearing practices. An analysis of the responses of the mothers of the 36 most prejudiced and of the 36 least prejudiced children indicates that the former tend toward greater authoritarianism and greater rigidity or "fussiness." Unfortunately, the returns from one school were so few that the effect of social class was not estimated. The authors emphasize that these interrelations of ethnic attitudes with other characteristics of the personality and the social environment must be considered in any program for changing attitudes. A study by Mussen (81) documents this assertion. One hundred and six white, lower class New York City boys, ages 8 to 14, were given the Horo-

XUM

witz Faces Test shortly before leaving the city for a four-week stay at a camp where there were an equal number of Negro boys with whom they would share all of the camp's activities without any formal segregation. The Faces Test was again administered just before the boys left the camp. In the meantime, each of the subjects was given the Thematic Apperception Test while at camp together with an interview about his feelings about the camp and the other campers. The group as a whole did not change in its attitude toward Negroes. There were 27 subjects who definitely increased in prejudice and 28 who showed definite decrease. Increase in prejudice during the four weeks was associated with need to defy authority, strong aggression feelings, belief that expression of aggression is followed by punishment, and unfavorable view of society in general, and dissatisfaction with the camp and fellow-campers. Contact with Negroes changed relevant attitudes one way or the other in accordance with motivational and belief systems already present in the child. These studies from the University of Minnesota and Yale University, following the important California volume on *The Authoritarian Personality* (3), are encouraging tokens of the increasing amount of knowledge and well-directed effort now being applied to the study of social attitudes in children.

Sociometric data obtained from children are reported in three studies which develop new methods and problems. Thompson, Bligh & Witryol (124) confront the fact that investigators may wish to obtain sociometric data for such purposes as assisting the adjustment of a child or of a group, developing better instruments and methods, or determining the relationship of status to other personality characteristics. On the basis of data obtained from eight sixth-grade classes, they provide a careful discussion of some of the decisions, as related to research aims, which must be made in the course of statistical analyses. Polansky, Lippitt & Redl supply two reports (89, 90) on the initial phases of an ongoing project investigating group treatment processes. Their focus was on the phenomenon of group contagion and on the relationships between social status in a group and the initiation of group behavior. Their cautious and full account of how they developed, in several summer camps for disturbed children, operational definitions of these concepts and discovered certain relationships is very thought-provoking. We look forward to their promised attempt to modify perception of group position and to their publication of the conceptual scheme within which these important kinds of data can be related.

Mass communications.—Butterworth & Thompson (19) have obtained the preferences for comic books of 1,458 school children, grades 5 to 12, in rural and urban schools. Distinct sex differences were found at all ages. Urban-rural differences were not found. Definite changes in preference were found with age in terms of specific titles; humor increased in appeal with age, while the appeal of cowboys and supermen decreased. Donald Duck and Bugs Bunny increased decidedly in popularity at the 12th grade! Wall & Simson

(126, 127) report on the effect of motion picture films on the social behavior and emotional response of several thousand English children, ages 13 to 16, as reported by the children. Riley & Flowerman (98) report on a project in which they consider the response of the individual in terms of his group membership. They develop three measures of the child's status in an intercommunicating group, reporting here specifically on the communications score. This is obtained through the number of times a child mentions his peers in a personal interview consisting of questions about the persons he talks his problems over with when he has to decide what to wear to a party, which movie to attend, how to get along with others, etc. Preliminary analysis of research with 400 school children indicates that peer-orientation influences selection of radio programs and comic books and response to the content.

The teacher and school.—Various studies of the relation of the class room to child behavior will, of course, be reported extensively elsewhere in this volume. We wish to review, however, several studies which contribute to an important problem in child psychology proper, that is, the conceptualization of social environment of the child with operational definitions embodying the social interactions of adult with child. An intriguing and brief report by Alexander (5) explores the possibility of predicting teacher behavior in the class room on the basis of her responses to a projective test consisting of eight pictures showing child-adult interaction. These protocols were rated on a three-point scale for seven categories, such as acceptance of fun-making, of childhood sexuality, and of aggression. Furthermore, various members of the staff of a larger research project collected data on the teachers' behavior in the class room with reference to the same categories and quantified on the same three-point scale. For all seven categories, 76 per cent of the predictions were in agreement with the observational data, a percentage significantly larger than the 36 per cent agreement expected by chance. The study is not reported in as full detail as is warranted by its obvious potential contribution to existing methods of measuring aspects of behavior important for social learning. Leeds (65) reports in handsome detail his patient development of a teacher-pupil inventory as an instrument for measuring teacher attitudes toward pupils and for differentiating those who get along well with children from those who do not. Its final form was administered to 100 teachers, unselected as to teacher-pupil relationship, on whom ratings were then obtained from principals, from observations in the class room, and from pupils. Correlations of the inventory score with the ratings were .43 with principals, .49 with observations, and .45 with pupils. We thus have a carefully defined statistical construct, teacher's personal effectiveness in the class room, a measure of which can be obtained in only 20 to 30 min.; the extent of its usefulness has yet to be determined.

Slobetz (111) obtained from 290 teachers, through an open-ended type of questionnaire, 11,600 reports involving 113 different types of practices

XUM

actually used in responding to various behavior problems in the class room. His report provides a good source of descriptive data on the "natural history" of the class room. The over-all ratio of constructive to punitive practices for all problems was about three to one; this ratio was lowest in dealing with problems of class room decorum and highest in dealing with withdrawing and recessive behavior. Simon & Thompson (109) find that elementary pupils predict that teachers will be punitive rather than constructive about 74 to 80 per cent of the time in responding to such problems as failure to do homework, chronic lateness, and cheating. They would prefer punishment much less frequently but explanations as to acceptable behavior and assistance in dealing with their problems much more frequently.

Witryol (133) goes much further than Slobetz (111), both in method and in number of problems attacked. After preliminary use of an open-ended questionnaire to obtain from teachers and students those behaviors they considered to be teacher-approved or disapproved, he used the method of paired comparisons with 2,240 sixth to twelfth grade students, 80 teachers, and 240 students of education to establish the scale values of the 10 most approved and 10 least approved behaviors mentioned in the questionnaire. The results showed a definite development with age in the ability of children to discriminate with respect to these social values. Twelfth graders rank the behaviors in high agreement with teachers and students of education, giving highest ranks for honesty, courtesy, and respect for others. One large disagreement was the higher rating teachers gave to co-operation. The author discusses ways in which the student's high evaluation of certain general conduct might be used by the teacher in responding to specific instances of approved and disapproved behavior. This point has important implications with respect to any attempt to measure the punishment- or reward-value of comments which adults make in response to children's behavior.

While the previous studies deal with measures of teacher behavior, Mensh & Mason (73) take two entire schools and classify one, a progressive school, as possibly less frustrating, the other, a traditional school, as possibly more frustrating. The Rogers Test of Personality Adjustment, given to 75 girls in the former school and 110 girls in the latter, indicated no significant difference in overall adjustment level. Furthermore the Rosenzweig Picture-Frustration test showed no significant differences in the students' reactions to frustration except with respect to overconformity in the traditional school students and underconformity in the other group.

Stendler & Young (116) and Stendler (115) report on the impact of first grade on the child and his parent. Two interviews with 212 mothers were made, one before the child entered first grade, the other after two months of school. In general, the mothers reported favorably on the child's experiences. Only a few of the lower-lower class mothers had sent their children to preschool, while all of the uppers and upper-middles had done so. The lower

class families had done less to prepare their children for school with regard to alphabet, nursery rhymes, etc., except with regard to learning to count. No social class differences were found in parental criticism of the school.

The family situation.—Interest in breast feeding continues, but for several different reasons, ranging from its effect on good facial growth to its relation to the oral character as described in psychoanalytic literature. Pottenger & Krohn (91) report that the early feeding experiences of 327 subjects in three age groups (under 12, 12 to 25, and over 25) are related to facial growth. Fifty-seven per cent of the subjects who were breast-fed had bimalar distance larger than their biorbital distance while only 10.5 percent of those not nursed showed this relationship. Two studies are concerned with breast feeding and some aspect of later behavior. Newton (82) related infant feeding experience to nursery school adjustment in 24 normal children. Although certain positive relationships between successful breast feeding and good total adjustment were found, there was no direct relationship between either amount or degree of breast feeding and later behavior. Both rigid and flexible schedule groups had more desirable behavior than the less rigid group. Average adjustment scores and average social adaptability scores of early and late weaned groups were about equal, and the suddenly-weaned group did not differ markedly from the total group, but the unheld group did. These facts, taken together with the fact that other investigators have found differing trends in different groups, cause the author to conclude that the differences found for the breast-fed group are meaningful only in terms of the mother's attitude toward child care. Goldman (41) came to a somewhat similar conclusion after studying the early feeding experiences and weaning of 100 adult subjects who had previously been scored for oral pessimism on the basis of verbal rating scales for 19 character traits and the type pattern derived therefrom. Although some positive relation between weaning earlier than four months and oral pessimism and weaning after nine months and oral optimism was found, it is concluded that other factors such as attitude of the mother, constitution of the child, and the interaction of both account for it to a greater degree.

That successful and unsuccessful breast feeding and weaning trauma are symptoms of more fundamental facts, such as the attitude of the mother, is given further credence by two further studies. Newton & Newton (83) interviewed 91 mothers in the maternity ward of a hospital before or within 24 hr. after delivery with regard to their wishes and expectations for breast feeding their infants. Responses were classified as positive, doutbful, and negative. It was found that 74 per cent classified as positive were able to breast feed successfully whereas only 35 per cent of the doubtful group and 26 per cent of the negative group breast fed successfully. Negative breast feeding attitudes tended to be allied with other negative attitudes such as unqualified discontent with sex of child and reports of extremely hard labor. Wallin & Riley (128), seeking to relate adjustment to pregnancy to later adjustment

of the infant, found no relationship in a limited group of 63 one-child cases but a significant relationship in 37 two-child cases. They propose three possible explanations, all of which seem closely related to the negative maternal attitude suggested in the above studies. On the basis of all these results, which should be considered suggestive rather than conclusive because of small samples and loose controls, it would seem that studies of breastfeeding, weaning, and attitude toward pregnancy are contributing to our knowledge of the social environment of the infant.

Concern with the total family situation as a force in the development of the personality of the child has been relatively great and varied, depending again on the particular orientation of the investigator. One group of studies is concerned with the personality or behavior of parents of disturbed children, with primary emphasis on correlation of certain parental characteristics with those of the child and only secondarily interested in the dynamics of the relationship. A second group, which interestingly enough is concerned primarily with therapy, analyzes the sociopsychological structure of the family as the prime force acting on the developing personality of the child. A third is concerned with the relation of actual behavior in children to practices in the home situation with a view to throwing light on the dynamics of this relationship.

At the purely correlational level, Roff (99) surveys the literature on intrafamily resemblances in a wide variety of personality characteristics in an attempt to estimate the extent to which developmental outcome for the variables reviewed can be predicted from appraisals of the same characteristic in other family members. Phillips (87), to test the proposition that children's psychological difficulties reflect and grow out of the parent's psychological difficulties, analyzed guidance center records of 52 cases for occurrence of 42 symptoms in children and in their parents. Seventy-one per cent of the symptoms showed communality among mother-child pairs and 48 per cent among father-child pairs. It was also noted that the complaints regarding the child which bothered the parent most were centered around unresolved psychological difficulties which the parents had in their own histories. While suggestive, these results should be evaluated in the light of the manner of selection of the cases from the total population and the well-known fallibility of routine clinical records which were not collected with research in mind. In a similar study making use primarily of routine case records selected from a larger sample, McKeown (71) categorized parental behavior of schizophrenic, neurotic, and normal children according to sex of parents. Parents were classified as demanding-antagonistic, superficial, encouraging, or protective-indulgent. The most prevalent type of behavior on the part of like-sex parents of both the schizophrenic and neurotic groups was that called demanding-antagonistic. Incidence of this behavior among parents of the opposite sex was slight among schizophrenic and normal groups but significantly greater among neurotic children. Encouraging behavior was

comparatively absent in parents of both sexes for both schizophrenic and neurotic groups. Morris & Nicholas (77), in a Rorschach study of intrafamilial personality configurations among 46 children and adolescents with primary behavior disorders, found significant homogeneity among all three family members (child and both parents) on 15 of 20 personality variables studied. Of interest in this study is the use of rating scales of personality variables based on Rorschach records.

Skodak (110), as an interesting side light on the longtime follow-up of children placed in adoptive homes, compares 22 pairs of adopted-and-own and 41 pairs of adopted-and-adopted children as a contribution to the problem of extent to which children in the same family resemble each other in mental growth. Although the data available are not as exhaustive as in the original investigation because of the unplanned nature of this study, results indicate that adopted children, unrelated to each other, but placed in a permanent home in infancy and brought up as siblings, are as similar in intelligence at comparable ages as are natural siblings. This relationship is less clear for adopted-and-own pairs. Although the author does not suggest it, the latter finding, in the light of some of the other investigations reported below, would suggest the influence of intrafamily dynamics in the area of mental growth.

Both Henry & Warson (50) and Ackerman & Sobel (2) assign prime importance to certain family patterns in the development of the personality of the child and in diagnosing and treating behavior problems. Henry & Warson are concerned with the various systems of interacting persons in the family. They find that they need social, cultural, and psychological concepts to describe these systems. One case is presented in detail as an example of diagnosis and therapy based on this approach. Ackerman & Sobel propose to define child personality as an expression of the sociopsychological configuration of the family unit. This approach requires a study of the family group going back to its original formation in the period of courtship and an understanding of the antecedents of the personal and social needs of the parents. They, too, present the diagnosis and therapy of one case in illustration. Relevant here is a research proposal of Winch (132) to describe and if possible predict the effect of the personality of the mother, the personality of the father, and the interaction of these personalities on the child.

An example of the third type of study mentioned above is reported in Hollenberg & Sperry (52). They report positive relationships between amount of frustration and of punishment in the home, as rated on the basis of mother interviews regarding child training practices in six areas of child training, and the frequency and intensity of aggressive behavior in doll play in 30 preschool children. They also report that amount of aggression in doll play can be modified by use of disapproval and of permissiveness. A similar study by Heathers (49) relates emotional dependence in preschool children to the behavior of their mothers. Data on the child's dependent and in-

XUM

dependent behavior, as observed both in nursery school activity and while performing special experimental tasks, were compared with simultaneous ratings of the mother's behavior on the Fels Parent Behavior Rating Scales by two staff home visitors. In general, high acceleration, democracy, and warmth in the mother are associated with low emotional dependence and high independence in the child's behavior, while high restrictiveness tended to be associated with high emotional dependence and hostility, and high indulgence with infantile forms of dependence. High child-centeredness was associated with some components of independence, such as initiative and noncompliance, but also with dependency in the form of seeking affection from the teacher.

Two studies which do not fall in the above groups are noted here. Stendler (114) analyzes child training practices as revealed in three major women's magazines over the last 60 years according to 10-year periods and finds that three different schools of thought as to how children should be raised have prevailed—a highly sentimental approach in the 1890's and 1900's, a rigid disciplinary approach from 1910 through the 1930's, and an emphasis on self-regulation and understanding of the child in the 1940's. The 60 years have also seen a swing from emphasis on character development to emphasis on personality development. Blum (17) studied various aspects of the pediatrician-mother and pediatrician-child relationships that have a bearing on the educational and psychological role of the pediatrician in the family.

Theory and Research

We had hoped to make a careful survey of the impact of psychological theory on research in child psychology and to use this as a co-ordinating focus for our report. The task, however, was too great for our time and space limits. It should have included at least that research which attempts to use psychoanalytic, Lewinian, and learning theory. There is need for such a survey, and undoubtedly it can be done much more adequately through inclusion of studies published over a longer span than that of the current year. Moreover, as has been apparent to the reader, most research in child psychology is distinguished by lack of reference to systematic theory.

To show certain important trends, we briefly comment here on only two large research projects in which theory played an important role. Wright & Barker (134) have privately published a progress report on their Midwest Field Study called *Methods in Psychological Ecology*. Their goal is the study of human behavior in naturally occurring situations; they wish to contribute to the general problem of measuring the parameters of psychological laws for different cultures and conditions of life. More specifically, they wish to describe the social environment of children in psychologically significant terms. A research station has been established in a small town, the actual aims and methods of the investigators have been made clear and understandable to the townspeople, and many of the staff now live there. Data are limited to

descriptive accounts by trained observers who make their reports according to various carefully formulated rules aimed at uniformity of form and fullness and objectivity of content. These records are found to be divisible into basic units, called episodes, a concept which resembles Mead's social act. These episodes are usually of fairly short duration; hundreds occur in the day of an average boy (9). Furthermore, the structure of episodes is such that various phases, sections, and stages may be identified despite complex and subtle overlappings and gaps. At this point, Wright & Barker describe the crucial part of their project: their use of 153 different categories in the coding of each of the episodes! This amazingly prolonged examination of small specimens is, to our knowledge, without parallel in behavioral science, although earlier work by Lewinian students was moving, for a time, in this direction. It may take years to answer obvious problems of reliability, validity, and usefulness. The authors show how Lewin's geometrical language helped solve the problem of discriminating molar units and how other Lewinian concepts helped in developing the categories they use for describing the environment. Finally, the coded episode is placed on IBM cards to facilitate the search for relationships among environmental and behavioral categories. It is here that we find the most serious difficulty in their present design. At the moment of observation, the Midwest observer has the explicit task of "seeing behavior and the situation which gives rise to it all at once" (134, p. 47). His report of the episode perpetuates his relating of this environmental factor to this behavior. He has been trained not to theorize in terms of professional jargon in his report, but still his judgment that this gave rise to that is accepted as part of each basic datum. We wonder whether this project can find laws which are any more general than the very low order laws which the observer assumed to be operating at the time he made his judgment and selected the data he assumed to be related; we also wonder about the degree to which such records are influenced by both individual and cultural bias. We shall await their evaluation of the various sources from which they will have derived testable hypotheses to guide them among the millions of intercorrelations at their disposal. The tone of their report is delightfully frank and promises easy communication between Midwest investigators and others.

A second research project involving theory was carried out at the Iowa Child Welfare Research Station from 1947 to 1949. Sears was project director of this investigation of relationships between maternal behavior and aggressive and dependent behavior in preschool children. An over-all report by him is forthcoming. Sears, Whiting & Nowlis, with seven collaborators (13, 20, 32, 38, 52, 70, 84, 103 to 106), began by attempting to translate certain key concepts in learning theory from the domain of simple behavior to a new empirical domain, that of the social behavior of middle-class midwesterners with their infants and small children. Since data on maternal behavior were to be secured through interview and data on child behavior through observation, ratings, and doll play, operational definitions of the key learning

XUM

concepts were tremendously involved; for example, the chain of operations with interview data consisted of the selection of informants, the design of the interview, the complicated interviewing process, record transcription, categorization of material, design of rating scales (punitiveness, infantile frustration, etc.), the rating process, and the various combinations of scales. Those who gathered data on children worked independently of those concerned with mother interviews. The source of testable hypotheses was the fairly systematic knowledge we have regarding stimulus-response laws. With such strategy, unsuccessful predictions (e.g., that maternal punitiveness is negatively related to amount of aggression in preschool) were as useful as successful ones (e.g., that high frustration at home is associated with high aggression in doll play). Present tactics at the University of Iowa and in the Harvard Laboratory of Human Development include both field studies and laboratory studies where antecedents are varied and behavior predicted. A rather crucial methodological problem in this kind of program involves the recruitment of evidence in deciding whether the failure of a particular hypothesis is due to (*a*) inadequate translation of a concept from simple situations to social situations, (*b*) failure to develop adequate instruments for these very difficult phenomena, or (*c*) modification of the law itself in these more complex situations. Perhaps the greatest advantage of such a program rests in the fairly direct way in which its findings contribute to the all-important search for laws of increasing generality.

Both the Barker & Wright and the Iowa-Harvard approaches should be encouraged, together with other ongoing attempts to apply theory to research on the problem of the impact of sociocultural factors on child development. While the former seems to have temporarily abandoned any constructive and corrective working relationship with theory for an overabundance of observational fact, the latter suffers from the persistent lag between development of theory and development of instrument. Many of the empirical studies included in this review may contribute in some way to the shortening of this lag. Fortunately for both types of projects, American adults are at present prepared to co-operate to rather unexpected lengths with scientists in research enterprises in which a personal relationship of mutual respect and understanding is achieved. Foreign visitors to these laboratories frequently comment differently about the attitudes of informants in their own country. We have here an important resource to which psychologists should probably give more thought.

Finally, we would endorse many of the negative evaluations of the area of child psychology made in this review last year. However, we know many psychologists who are chronically dissatisfied with the current status of psychology as science but who are productive because of their strong determination to do the best possible job under admittedly difficult circumstances. Perhaps a special weakness of child psychology is that such determination frequently keeps the careful investigators in other areas of psychology. To

the extent that those interested in child psychology can combine rigorous investigation with readiness to tackle disturbingly complex problems, the vitality of child psychology as a field in behavioral science is assured.

Surveys and Theoretical Discussions

Some of the studies which were important in the history of child psychology are brought together by Dennis (26). Very helpful discussions of growth and maturation are found in Carmichael (21), Shock (108), and Stone (119). A broad review of sexual development is included by Ford & Beach in their *Patterns of Sexual Behavior* (35). The following surveys are of interest: Goodenough & Harris on the psychology of children's drawings (42), Donahue on research for the blind (29), Massler *et al.* on thumb-sucking (66) and nail-biting (67), Hildreth on lateral dominance (51), Genn & Genn (36) on primary emotions in neonates, and Mensh on Rorschach studies of gifted children (72); an entire issue of the *Review of Educational Research* is devoted to the area of growth and development every three years (97). Stoke (118) has written a critical review of the concept of identification, and Pinneau (88) carefully reviews the articles of Ribble. The analysis by Dollard & Miller (28) of the learning of normality in psychotherapy describes various relationships which they repeatedly apply to the social development of the child. Mowrer (78) also applies learning theory to such problems as enuresis, language development, and identification. McClelland (69), from a similar viewpoint, provides an analysis of the socialization process. The fifth volume of *The Psychoanalytic Study of the Child* (92) contains a symposium on the relation of psychoanalysis to developmental psychology.

XUM

LITERATURE CITED

1. Abel, T. M., and Joffe, N. F., *Am. J. Psychother.*, **4,** 90–113 (1950)
2. Ackerman, N. W., and Sobel, R., *Am. J. Orthopsychiat.*, **20,** 744–53 (1950)
3. Adorno, T. W., Frenkel-Brunswik, E., Levinson, D. J., and Sanford, R. N., *The Authoritarian Personality* (Harper & Brothers, New York, 990 pp., 1950)
4. Alberts, E., and Ehrenfreund, D., *J. Exptl. Psychol.*, **41,** 30–38 (1951)
5. Alexander, T., *J. Clin. Psychol.*, **6,** 273–76 (1950)
6. Ammons, R. B., *J. Genetic Psychol.*, **76,** 323–41 (1950)
7. Ausubel, D. P., *Genetic Psychol. Monographs*, **43,** 53–117 (1951)
8. Banham, K. M., *J. Genetic Psychol.*, **76,** 283–89 (1950)
9. Barker, R. G., and Wright, H. F., *One Boy's Day* (Harper & Brothers, New York, 435 pp., 1951)
10. Barnouw, V., *Acculturation and Personality Among the Wisconsin Chippewa* (Mem. Series 72, Am. Anthrop. Assoc., Menasha, 152 pp., 1950)
11. Bayley, N., *Child Development*, **22,** 47–60 (1951)
12. Beier, E. G., Gorlow, L., and Stacey, C. L., *Am. J. Mental Deficiency*, **55,** 582–89 (1951)
13. Beller, E. K., *Dependency and Independence in Young Children* (Doctoral thesis, State Univ. of Iowa, Iowa City, 101 pp., 1948)
14. Benjamins, J., *J. Abnormal Social Psychol.*, **45,** 473–80 (1950)
15. Berenda, R. W., *The Influence of the Group on the Judgments of Children* (King's Crown Press, New York, 86 pp., 1950)
16. Blomquist, B. L., *J. Speech Hearing Disorders*, **15,** 159–64 (1950)
17. Blum, L. H., *Genetic Psychol. Monographs*, **41,** 3–97 (1950)
18. Broida, D. C., Izard, C. E., and Cruickshank, W. M., *J. Clin. Psychol.*, **6,** 243–48 (1950)
19. Butterworth, R. F., and Thompson, G. G., *J. Genetic Psychol.*, **78,** 71–96 (1951)
20. Carl, L. J., *An Experimental Study of the Effects of Nurturance on Preschool Children* (Doctoral thesis, State Univ. of Iowa, Iowa City, 77 pp., 1949)
21. Carmichael, L., *Handbook of Experimental Psychology*, 281–303 (Stevens, S. S., Ed., John Wiley & Sons, Inc., New York, 1436 pp., 1951)
22. Centers, R., *J. Personality*, **18,** 290–302 (1950)
23. Clark, K. B., and Clark, M. P., *J. Negro Educ.*, **19,** 341–50 (1950)
24. Crowder, T. H., *Experimental Modification of Response to Frustration in Preschool Children* (Doctoral thesis, State Univ. of Iowa, Iowa City, 80 pp., 1949)
25. Dennis, W., *Perception: An Approach to Personality*, 148–69 (Blake, R. R., and Ramsey, G. V., Eds., The Ronald Press Co., New York, 442 pp., 1951)
26. Dennis, W., Ed., *Readings in Child Psychology* (Prentice-Hall, Inc., New York, 624 pp., 1951)
27. Devereux, G., *Psychiatry*, **13,** 489–502 (1950)
28. Dollard, J., and Miller, N. E., *Personality and Psychotherapy* (McGraw-Hill Book Co., Inc., New York, 488 pp., 1950)
29. Donahue, W., *Outlook for Blind*, **44,** 31–41 (1950)
30. Dubo, S., *Am. J. Orthopsychiat.*, **20,** 520–28 (1950)
31. Erikson, E. H., *Childhood and Society* (W. W. Norton & Co., Inc., New York, 397 pp., 1950)

XUM

32. Faigin, H., *Frequency and Type of Rules Used by Preschool Children in Their Social Interaction* (Master's thesis, State Univ. of Iowa, Iowa City, 62 pp., 1948)
33. Feifel, H., and Lorge, I., *J. Educ. Psychol.*, **41**, 1–18 (1950)
34. Fitzgerald, D. C., *Quart. J. Personality*, **1**, 67–83 (1951)
35. Ford, C. S., and Beach, F. A., *Patterns of Sexual Behavior* (Harper & Brothers, New York, 307 pp., 1951)
36. Genn, G., and Genn, M. M., *Quart. J. Child Behavior*, **1**, 439–46 (1950)
37. Gesell, A., and Ames, L. B., *J. Pediat.*, **36**, 165–76 (1950)
38. Gewirtz, J. L., *Succorance in Young Children* (Doctoral thesis, State Univ. of Iowa, Iowa City, 120 pp., 1948)
39. Goff, R. M., *J. Negro Educ.*, **19**, 152–58 (1950)
40. Goldenberg, S., *J. Speech Hearing Disorders*, **15**, 221–33 (1950)
41. Goldman, F., *J. Personality*, **19**, 189–96 (1950)
42. Goodenough, F. L., and Harris, D. B., *Psychol. Bull.*, **47**, 369–433 (1950)
43. Gorer, G., and Rickman, J., *The People of Great Russia* (The Cresset Press, London, 236 pp., 1949)
44. Gough, H. G., Harris, D. B., Martin, W. E., and Edwards, M., *Child Development*, **21**, 83–91 (1950)
45. Harris, D. B., *J. Consulting Psychol.*, **14**, 381–85 (1950)
46. Harris, D. B., *Intra-child vs. Inter-child Consistency in the Drawing of a Man* (Presented at Am. Psychol. Assoc., State College, Penn., 1950)
47. Harris, D. B., Gough, H. G., and Martin, W. E., *Child Development*, **21**, 169–81 (1950)
48. Harris, I. D., Rapoport, L., Rynerson, M. A., and Samter, M., *Am. J. Orthopsychiat.*, **20**, 490–505 (1950)
49. Heathers, G., *Emotional Dependence and Independence in Preschool Children* (Presented at Society for Research in Child Development, Detroit, Mich., 1951)
50. Henry, J., and Warson, S., *Am. J. Orthopsychiat.*, **21**, 59–73 (1950)
51. Hildreth, G., *J. Genetic Psychol.*, **76**, 39–144 (1951)
52. Hollenberg, E., and Sperry, M., *Quart. J. Personality*, **1**, 32–43 (1951)
53. Honzik, M. P., *Child Development*, **22**, 15–35 (1951)
54. Huang, I., and Lee, H. W., *J. Genetic Psychol.*, **66**, 69–74 (1945)
55. Ilg, F. L., and Ames, L. B., *J. Genetic Psychol.*, **76**, 291–312 (1950)
56. Irwin, O. C., *J. Speech Hearing Disorders*, **16**, 159–61 (1951)
57. James, H. E. O., and Tenen, C., *Brit. J. Psychol., Gen. Sect.*, **41**, 145–72(1950)
58. Jervis, F. M., and Haslerud, G. M., *J. Psychol.*, **29**, 67–76 (1950)
59. Johnson, G. O., *Am. J. Mental Deficiency*, **55**, 60–89 (1950)
60. Johnson, G., *J. Educ. Psychol.*, **41**, 257–78, 428–39 (1950)
61. Jones, M. C., and Bayley, N., *J. Educ. Psychol.*, **41**, 129–48 (1950)
62. *Journal of Negro Education*, **19**, 215–425 (Thompson, C. H., Ed., The Bureau of Educational Research, Howard University, Washington, D. C., 1950)
63. Keister, M. E., *J. Am. Dietet. Assoc.*, **26**, 25–29 (1950)
64. Kuenne, M. R., *J. Exptl. Psychol.*, **36**, 471–90 (1946)
65. Leeds, C. H., *Psychol. Monographs*, **64**(6), 1–24 (1950)
66. Massler, M., and Wood, A. W. S., *J. Dentistry Children*, **16**, 1–9 (1949)
67. Massler, M., and Malone, A. J., *Am. J. Orthodontics*, **36**, 351–67 (1950)

XUM

68. McCary, J. L., *Quart. J. Personality*, **1**, 84–102 (1951)
69. McClelland, D. C., *Personality* (William Sloane Associates, New York, 654 pp., 1951)
70. McKee, J. P., *The Relationship Between Maternal Behavior and the Aggressive Behavior of Young Children* (Doctoral thesis, State Univ. of Iowa, Iowa City, 67 pp., 1949)
71. McKeown, J. E., *Am. J. Sociol.*, **56**, 175–79 (1950)
72. Mensh, I. N., *J. Except. Children*, **17**, 8–14 (1950)
73. Mensh, I. N., and Mason, E. P., *J. Educ. Research* (In press)
74. Metraux, R. W., *J. Speech Hearing Disorders*, **15**, 37–53 (1950)
75. Miller, H., and Baruch, D. W., *Annals of Allergy*, **8**, 100–7 (1950)
76. Miller, H., and Baruch, D. W., *Am. J. Orthopsychiat.*, **20**, 506–19 (1950)
77. Morris, W. W., and Nicholas, A. L., *J. Clin. Psychol.*, **6**, 309–19 (1950)
78. Mowrer, O. H., *Learning Theory and Personality Dynamics* (The Ronald Press Co., New York, 776 pp., 1950)
79. Mummery, D. V., *Child Development*, **21**, 183–96 (1950)
80. Murdock, G. P., Ford, C. S., Hudson, A. E., Kennedy, R., Simmons, L., and Whiting, J. W. M., *Outline of Cultural Materials* (Human Relations Area Files, Inc., New Haven, 162 pp., 1950)
81. Mussen, P. H., *J. Abnormal Social Psychol.*, **45**, 423–41 (1950)
82. Newton, N. R., *J. Pediat.*, **38**, 28–40 (1951)
83. Newton, N. R., and Newton, M., *Pediatrics*, **5**, 869–75 (1950)
84. Nowlis, V., *Am. J. Orthopsychiat.* (In press)
85. Paul, B. D., *Am. Anthropologist*, **52**, 205–18 (1950)
86. Phillips, E. L., *J. Genetic Psychol.*, **77**, 61–72 (1950)
87. Phillips, E. L., *J. Clin. Psychol.*, **7**, 188–90 (1951)
88. Pinneau, S. R., *Child Development*, **21**, 203–28 (1950)
89. Polansky, N., Lippett, R., and Redl, F., *Sociometry*, **13**, 39–62 (1950)
90. Polansky, N., Lippett, R., and Redl, F., *Human Relations*, **3**, 319–48 (1950)
91. Pottenger, F. M., and Krohn, B., *Arch. Pediat.*, **67**, 454–61 (1950)
92. *Psychoanalytic Study of the Child*, **V** (Eissler, R. S., *et al.*, Eds., International Universities Press, New York, 410 pp., 1950)
93. Rabban, M., *Genetic Psychol. Monographs*, **42**, 81–158 (1950)
94. Radke, M., Sutherland, J., and Rosenberg, P., *Sociometry*, **13**, 154–71 (1950)
95. Radke, M. J., and Trager, H. G., *J. Psychol.*, **29**, 3–33 (1950)
96. Radke, M. J., Trager, H. G., and Davis, H., *Genetic Psychol. Monographs*, **40**, 327–447 (1949)
97. *Review of Educational Research*, **20**, 341–410 (Greene, H. A., Ed., Am. Educ. Research Assoc., Washington, D. C., 1950)
98. Riley, M. W., and Flowerman, S. H., *Am. Sociol. Rev.*, **16**, 174–80 (1951)
99. Roff, M., *J. Psychol.*, **30**, 199–227 (1950)
100. Rosenzweig, S., and Mirmow, E. L., *J. Personality*, **18**, 306–14 (1950)
101. Schonfeld, W. A., *Psychosomat. Med.*, **12**, 49–54 (1950)
102. Schuessler, K. F., and Strauss, A., *Am. Sociol. Rev.*, **15**, 752–62 (1950)
103. Sears, P. S., *Am. Psychologist*, **3**, 263 (1948)
104. Sears, R. R., *Am. Sociol. Rev.*, **15**, 397–401 (1950)
105. Sears, R. R., *Child Development*, **21**, 5–6 (1950)
106. Sears, R. R., *Am. J. Orthopsychiat.* (In press)

XUM

107. Sherman, M., and Bell, E., *Quart. J. Personality*, **1,** 44–53 (1951)
108. Shock, N. W., *Handbook of Experimental Psychology*, 330–346 (Stevens, S. S., Ed., John Wiley & Sons, Inc., New York, 1436 pp., 1951)
109. Simon, R., and Thompson, G. G., *Quart. J. Child Behavior*, **22,** 86–97 (1950)
110. Skodak, M., *J. Genetic Psychol.*, **77,** 3–9 (1950)
111. Slobetz, F., *J. Educ. Research*, **44,** 81–90 (1950)
112. Spitz, R., *Arch. Neurol. Psychiat.*, **63,** 467–70 (1950)
113. Springer, D. V., *Genetic Psychol. Monographs*, **41,** 215–70 (1950)
114. Stendler, C. B., *J. Pediat.*, **36,** 122–34 (1950)
115. Stendler, C. B., *Child Development*, **22,** 37–46 (1951)
116. Stendler, C. B., and Young, N., *Child Development*, **21,** 241–60 (1950)
117. Stiles, F. S., *J. Educ. Research*, **43,** 516–24 (1950)
118. Stoke, S. M., *J. Genetic Psychol.*, **76,** 163–89 (1950)
119. Stone, C. P., *Comparative Psychology*, 30–61 (Stone, C. P., Ed., Prentice-Hall, Inc., New York, 525 pp., 1951)
120. Stone, G. R., and Lynn, J. O., *J. Genetic Psychol.*, **78,** 97–103 (1951)
121. Strauss, A. L., *J. Genetic Psychol.*, **78,** 105–113 (1951)
122. Symonds, P. M., *Adolescent Fantasy* (Columbia Univ. Press, New York, 397 pp., 1949)
123. Thetford, W. N., Molish, H. B., and Beck, S. J., *J. Projective Techniques*, **15,** 58–78 (1951)
124. Thompson, G., Bligh, H., and Witryol, S. L., *J. Social Psychol.*, **33,** 13–32 (1951)
125. Townsend, E. A., *Genetic Psychol. Monographs*, **43,** 3–51 (1951)
126. Wall, W. D., and Simson, W. A., *Brit. J. Educ. Psychol.*, **19,** 53–61 (1949)
127. Wall, W. D., and Simson, W. A., *Brit. J. Educ. Psychol.*, **20,** 153–63 (1950)
128. Wallin, P., and Riley, R. P., *Am. J. Orthopsychiat.*, **20,** 616–22 (1950)
129. Weider, A., and Noller, P., *J. Clin. Psychol.*, **6,** 319–25 (1950)
130. Werner, H., and Kaplan, E., *J. Psychol.*, **29,** 251–57 (1950)
131. Whiting, B. B., *Paiute Sorcery* (Viking Fund, Inc., New York, 110 pp., 1950)
132. Winch, R. F., *Social Forces*, **28,** 310–16 (1950)
133. Witryol, S. L., *Genetic Psychol. Monographs*, **41,** 271–325 (1950)
134. Wright, H. F., and Barker, R. G., *Methods in Psychological Ecology* (Ray's Printing Service, Topeka, 276 pp., 1950)
135. Zeligs, R., *J. Genetic Psychol.*, **76,** 145–61 (1950)
136. Zeligs, R., *J. Genetic Psychol.*, **77,** 73–83 (1950)

XUM

LEARNING[1]

By Harry F. Harlow

Department of the Army, Washington, D. C.

In an important and challenging paper, Skinner (100) has raised the question of the value of learning theory, pointing out that learning theories may lead us to talk about manipulable variables without measuring them, may lead to much useless experimentation, and may hinder rather than help the design of experiments. Preoccupation with theory makes "particular observations seem too trivial," and the investigator comes to use "theory as a refuge from the data" (100, p. 195). An essentially similar position is taken by Brogden (13).

Although Meehl & MacCorquodale (67) are by no means opposed to theory, they take a catholic attitude toward the kind of theories that psychology might develop and the kind of constructs that might profitably be used. In criticizing some currently used constructs, they note, for example, that (p. 232)

> . . . r_g at present is so poorly quantified a construct that it is as readily available a *deus ex machina* for non-expectancy theorists as the concepts of "attention," "emphasis," or "perceptual threshold" are for Tolman.

The system-makers, who probably outnumber the skeptics, continue to present postulates, derive equations, and invent an ever increasing number of constructs in an effort to devise a theory that will encompass all the facts of learning. For example, Seward (90) not only accepts the construct of the antedating goal reaction (rs_G) but introduces the additional construct of tertiary motivation ($_sH_{rs_G}$) and an hypothesis of reinforcement based on tertiary motivation (90, p. 370): "*a response is reinforced when it is motivated by rs_G and rs_G is facilitated.*" In a detailed review of the literature on properties of reward and changes in properties of reward (latent learning), Seward (91) describes the role he would accord to tertiary motivation.

Hull (50) has made formal presentation of a theory of discrimination learning designed to show how simple qualitative discrimination learning evolves as differential reinforcement gradually builds up. Although such a theory may be useful in explaining discrimination learning in the naive rat, the existent experimental data on discrimination learning (39, 68, 69) have made archaic this and other existing stimulus-response (S-R) theories in this area.

On the basis of various postulates of behavior theory and the neurophysiological principle of summation, Taylor (108) deduced 10 postulates concerning reaction velocity and obtained reasonable relationships with experi-

[1] The survey of the literature to which this review pertains was completed in May, 1951.

mental data on the jumping-reaction latencies in rats. Ideal equations of forgetting (61) and forgetting following overlearning (62) have been derived by London, who, by the assumption that there are neurological processes that reactivate the inactivated units of engram-complexes, accounts for the fact that the decay functions characteristically level off above zero.

A qualitative theory relating and integrating conditioning, discrimination learning, conditional or multiple-sign learning, concept formation, and thinking has been presented by Harlow (41), and a very similar type of formulation is implicit in Nissen's chapter (78) in the *Handbook of Experimental Psychology*. In contrast to such integrative attempts, Mowrer & Lamoreaux (75) would place conditioning and problem solving at different levels of neural integration. But inasmuch as all conditioned responses (CR's) are discriminative to some degree and the autonomic and central nervous systems form a single great integrative mechanism, the presented data need not be interpreted as establishing different levels of reasoning. Weise & Bitterman (118) found that 10 rats run on a successive discrimination were superior to 10 others run on a simultaneous discrimination. On the basis of these atypical results obtained on a single species in a single experiment, the authors (p. 194)

> suggest the operation of two qualitatively distinct levels of perceptual organization in discriminative learning—a concrete, primitive, intra-situational level and a more abstract, selective, transcontextual one.

A striking fact in their experiment is the poor performance of the animals on the simultaneous problem compared to the performance obtained by other investigators on similar tests.

Caldwell (21, p. 14) has advanced a theory of adaptive conditioning which

> proposes that the distinction between instrumental conditioning and classical conditioning on the basis of the manner of reinforcement is fallacious because in all conditioning some stimulus pattern is reinforced—that is, reduced or terminated—by some response pattern of the organism.

Voeks (117) has presented a set of postulates and definitions based on Guthrie's learning theory.

Reviews of Learning

A wealth of excellent critical reviews of the field of learning has been published during the last year. In his chapter on learning in Volume 2 of the *Annual Review of Psychology*, Buxton (19) discussed the contributions from the point of view of S-R theory. The presentation is, however, objective and essentially impartial. Munn's recent book (76) devoted over 200 pages to the learning of the rat, and this scholarly and critical survey is an invaluable contribution to fact and theory. Chapters by Hilgard (46), Brogden (14), Hovland (48), Spence (103), Leeper (59), Morgan (72), Miller (70), and Nissen (78) in the *Handbook of Experimental Psychology* present both summarizing and interpretive material on learning. Chapters by Heron (45)

XUM

and Harlow (42) in the revised *Comparative Psychology* summarize learning in subprimates and primates, and a chapter by Spence (102) summarizes learning theories. The collected papers of Mowrer (74) have been presented in book form.

Latent Learning

In the last few years, interest in the long-latent, latent learning problem has shown a marked revival, springing not from the intrinsic merit or importance of the problem but from a consideration of latent learning as a "critical" test of S-R versus stimulus-stimulus (S-S) theory. The reincarnation of this problem, the type of experiment which it has engendered, and the theoretical entanglements which have resulted illustrate both the value of theory as expressed by the theoretically inclined and the limitations as enumerated by Skinner and Brogden.

A summary of the literature from the California point of view has been presented in a critical and evaluative review by Thistlethwaite (110), who appraises published latent learning material up to and partially through 1950 and some unpublished California studies. Spence, Bergmann & Lippitt (104) present a detailed discussion of an irrelevant-incentive study and interpret the results of this and other studies in terms of reinforcement theory. Seward, Levy & Handlon (92) report a study of incidental learning in the rat, also critically survey the literature, and come to the completely eclectic conclusion that "both reinforcement and cognition theories can make room for the kind of learning under discussion" (p. 248). Deese (25) has recently offered an alternative Hullian-type theoretical explanation of latent learning.

As the latent learning literature has expanded, need for classification of the problems has arisen, and Thistlethwaite (110) and Seward, Levy & Handlon (92) suggest strikingly similar classifications. For both, latent learning is learning, under drive, of paths without a goal object relevant to the drive, as in the classical Blodgett experiment. Thistlethwaite classifies, as a second variety of latent learning study, those experiments in which animals explore and live in empty mazes for brief periods of time. Irrelevant-incentive (or incidental) learning embraces two types of studies: (*a*) experiments in which the training is on mazes containing two incentives for which the subjects have been satiated, and (*b*) experiments in which subjects are trained on mazes having one path baited with an incentive appropriate to the animal's drive state and another path baited with an incentive for which the subject has been satiated.

Thistlethwaite and Seward, Levy & Handlon agree that latent learning is well established experimentally and that learning under satiation for both incentives is fairly certain under favorable conditions. Irrelevant-incentive learning under drive for one incentive and satiation for the other incentive is regarded as uncertain by these reviewers. Seward, Levy & Handlon evaluate the evidence as being weighted against learning under these conditions.

Strong drive is especially unfavorable for obtaining evidence of irrelevant-incentive learning.

A number of studies reported in the past year have broad theoretical orientation. Spence, Bergmann & Lippitt (104) trained food- and water-satiated rats on a Y-maze containing water in one goal box and food in the other. At the completion of a run, the subject was placed in a cage containing cage mates. Tests of learning were conducted under satiation for only one goal object. Appropriate choice was made on the initial test trial by hungry rats, but not by thirsty rats. Subsequently, Maltzman (63) conducted a similar experiment except that his subjects were required to run through goal boxes containing food or water, then through a short goal alley, and on to a social cage. Maltzman's subjects did not respond appropriately to the side containing the incentive for which they were tested on the first test day.

Application of the principle of competing anticipatory goal responses is presented by Spence, Bergmann & Lippitt and by Maltzman for both the positive and negative data of the two experiments. It is suggested that in the Maltzman study the shorter delay in obtaining social reward produced a stronger fractional anticipatory goal response for social reward and permitted this response to compete successfully and to interfere with the fractional eating and drinking responses. Against such an interpretation, Thistlewaite (110, p. 124) cites failure of nonhungry rats to select a food alley in preference to a nonfood alley and the inconsistency of the interpretation with the negative results of a number of other irrelevant-incentive experiments. Spence, Bergmann & Lippitt also recognize the many contradictory results of the various irrelevant-incentive experiments, but they take the position that these contradictory findings pose a particularly difficult problem for Tolman's motivational theory of perception.

Seward, Levy & Handlon (92) tested incidental learning in the rat by measuring the relative ease of habit reversal by satiated and by appropriately motivated animals. Rats were given preliminary training to water in the preferred direction on a T-maze and then divided into a control group, a thirsty group, a weak-drive group, and a satiated group. The experimental groups were given forced trails (half to each side in an irregular order) in each of three sessions. Finally, all groups were put on a 22-hr. thirst schedule and given five free trials a day for three days, with the position of the incentive reversed. The satiated and weak-drive groups were significantly better than the control group and worse than the drive group on the first test day, and on the first trial of the first test day the order of efficiency was drive group, weak-drive group, satiated group, and control group. The authors affirm that their results are not crucial for any specific form of learning theory but "point certain directions that each theory must take if it is to encompass them" (92, p. 248). In accounting for the incidental learning, reinforcement theorists are asked to state explicitly that a secondary reinforcing agent does not depend for its effectiveness on the presence of the drive to which it owes its power. They must explain why secondary reinforcement

XUM

presumably did not occur in various irrelevant-need experiments. The cognition theorist is requested, in view of the superiority of the drive group, to "give more explicit recognition than heretofore of drive as a factor determining the organization of perception" (92, p. 249).

Although the latent learning problem was at one time held by many to offer a critical test of the validity of cognition as opposed to reinforcement theory, it now appears that the problem threatens to effect an eventual synthesis. At least, theorists of both camps recognize the limitations of their theories in predicting latent learning results. Thistlethwaite states (110, p. 125): "There is no reason methodologically why theorists should not modify their formulations so as to be in closer agreement with empirical findings." Spence, Bergmann & Lippitt affirm (104, p. 550): "It is readily apparent that this S-R theory, like the rival sign-gestalt theory of Tolman, is much in need of more specific detailed elaboration." Kendler & Kanner (52, p. 765) state: "The present authors feel that there is no basic qualitative difference between a perceptual and S-R 'reinforcement' theory."

A number of relatively specific contributions to the latent learning problem area have appeared in the last year. A test was made by Kendler & Kanner (52) of Woodworth's suggestion that perceptual responses might have been learned but extinguished in Grice's and in Kendler & Mencher's irrelevant-motivation experiments. The precise and highly controlled technique used by Kendler & Mencher was modified by introduction of food on only the second day of two test days, and the effect of the disputed use of forced trials was tested by allowing half of each subject group free choices on all trials, massed to induce as much spontaneous alternation as possible. Careful analysis of the data gave no indication that the subjects had learned the location of the food—a finding consistent with results previously found in this specific experimental situation. The authors cautiously conclude (52, p. 765) that the results "do *not* imply perceptions may not be experimentally extinguished, nor do they preclude an adequate perceptual-reinforcement formulation."

In an experiment designed to test the role of drive externalization in the production of the Blodgett effect, Meehl & MacCorquodale (66) attempted a repetition of the Blodgett latent learning experiment, running an additional latent group which was fed one time in an extra-maze box (a maze-like situation) physically separate from the maze. This single feeding was given on the same day that the maze-latent group was first fed in the maze. No significant difference in rate of maze learning was demonstrated between the controls and either the maze-latent or box-latent group, but the experimenters emphasize that so much learning was manifested during the "unrewarded" trials that latent learning would have been somewhat difficult to demonstrate. Meehl & MacCorquodale point out that their data are similar to those obtained by Reynolds in regard to the amount of manifest learning shown by the latent groups. The use of wet mash as incentive would preclude Blodgett's criticism of Reynolds' experiment.

Evidence of latent learning under conditions of high motivation is pre-

sented by Strange (105), who ran his thirsty experimental group for 12 days, four trials a day, on a maze having food in one wing and nothing in the other wing. When switched to food deprivation, the experimental group was superior to a control group which had undergone the same preliminary training but had had no intervening maze experience. Inasmuch as the experimental group had both more, and more recent, general maze adaptation than the control group, the conclusions are open to question.

Failure to obtain latent learning after shift of high motivation in animals trained in an elevated maze to incentives appropriate to both motivational states is reported by Shaw & Waters (93). The flood of latent learning experiments which have engulfed, and will continue to engulf, the comparative psychology literature makes it clear that rats can learn in the absence of primary drive reduction. The data also make it perfectly apparent that the present-day rubber band theoretical systems can stretch to encompass any data regardless of how opposed they may be to the original predictions of the theorist.

Reinforcement and Learning

Drive and learning.—Latent learning is, of course, only one phase of the broad, general problem of the roles of drive state and incentive conditions in learning and performance. The effects of drive and drive reduction on conditioning, extinction, habit strength, and excitatory potential continue to occupy an important place in the psychological literature.

The accumulating evidence makes it appear more and more questionable that drive level and theoretical amount of drive reduction have any consistent, precise relationship to habit strength. Data of Strassburger (106) indicate that hunger drive level does not influence the strength of the bar-pressing habit as measured by subsequent resistance to extinction. Rats were given 1, 10, or 30 reinforced trials under food deprivation ranging from ½ hr. to 47 hr., and habit strength was measured in terms of number of responses, rate, and time to extinction under 23-hr. deprivation. Rate of extinction was inversely related to frequency of reinforcement, but no uniform and consistent relationship was found between food deprivation and habit strength as measured by extinction. These data contrast with those previously reported for rats by Finan (29), but they are in accord with those reported by Meyer (68) for discrimination and discrimination-reversal learning in the monkey. Meyer found no evidence that food deprivation from 1 to 47 hr. affected habit formation in the monkey.

Taylor (107) presents data which she interprets as indicating that two different conditions of drive (anxiety) level influence the rate of development of the conditioned wink response in human subjects. Anxiety level was measured by the Minnesota Multiphasic Personality Inventory. The anxious (high score) group was consistently and significantly superior to the non-anxious (low score) group in amount of conditioning, but attempts to influence subgroups by anxiety-raising or anxiety-reducing instructions

XUM

failed. Anxious subjects showed greater, but not significantly greater, resistance to extinction. It is pointed out that the results might be interpreted either as indicating a differential rate in the development of habit strength or merely a differential rate in the growth of the curve of excitatory potential.

Whether or not habit strength is influenced by the strength of the drive state at the time of habit formation, there is evidence to show that the strength of response tendencies toward food is related to the concurrent deprivation state. Horenstein (47) measured the resistance to extinction of a panel-pushing habit and the gross food intake of rats. A sharp rise was found in response measures during deprivation periods between 0 and 2 hr., and a much more gradual increase obtained from 2 to 23.5 hr. The data are interpreted as indicating "in addition to a stimulus generalization gradient around 23.5 hr., the presence of another specific factor operative around 2 hr. of deprivation" (47, p. 222). An alternative explanation in terms of the digestive mechanisms of the rat is also offered. Horenstein's results support published work by Koch & Daniel (55) and Saltzman & Koch (88) and unpublished data by Yamaguchi.

Once a habit has formed, efficient performance may persist under conditions of low or "zero" drive. Zeaman & House (121) tested the functional relationship between the strength of responses as measured by log latency under zero drive and the number of previous reinforcements under maximum drive. Drive was induced by differential illumination of a two-compartment box, and the condition for zero drive was a level of illumination originally found to be incapable of producing the learned escape response. At this level of zero drive the strength of the performance plotted as an increasing, negatively accelerated function of the number of previous reinforcements under maximum drive. The results, taken to be a direct confirmation of Hull's Corollary II (49, p. 247), are, as the authors point out, also consistent with Anderson's theory of externalization of drive, Allport's functional autonomy, and Woodworth's construct of mechanisms functioning as drives.

Another ingenious technique for demonstrating response evocation under conditions of zero drive is described by Teel & Webb (109), who ran 84 rats on a single-unit T-maze for 14 days. On the last 13 days the animals had four trials, two free and two forced, under 23-hr. hunger, after which they were satiated and run two additional trials, one free and one forced. Performance on the free nonsatiated trials and free satiated trials was approximately equal throughout the 13 test days and reached 98.5 per cent correct responses on the free-choice satiation trials of the last two days. These data clearly show that a high performance level may be maintained, at least transiently, under drive satiation, and they by no means exclude the possibility of habit formation during the satiation trials.

Additional evidence indicative of the complexity of relationships that exist between reinforcement and acquisition is presented by Kendler &

XUM

Law (53), who compared the performance of two groups of rats, one motivated solely by a relevant drive (hunger or thirst), the other by a combined relevant-irrelevant drive, on a positional discrimination reversal. The reversed response was motivated by a drive state and incentive appropriate to the drive previously absent, or present but irrelevant. No difference obtained in the reversal scores of the two groups, and these results are interpreted as being consistent with Kendler's selective principle of association of drive stimuli (53, p. 299): " . . . only those drive stimuli which are themselves reduced become connected to a rewarded response."

The effect of an irrelevant need on total drive strength was tested by Amsel & Maltzman (5) by training rats to drink in individual cages until daily water consumption in a 10-min. period was relatively constant. Then, for three days the animals were subjected before drinking to a series of shock trials in a different situation. Mean water intake showed a significant rise, interpreted as indicating an increase in total generalized drive strength. The effects of pain and anxiety on thirst (inhibition of salivation) are discounted.

In a subsequent experiment Amsel (4) established stable drinking habits in 25 rats in a double-compartment, grilled-floor apparatus, then divided the subjects into experimental and control groups. The experimental subjects were given four daily shock sessions in the compartment without the drinking device. Subsequent water intake in the apparatus dropped almost to zero for the experimental subjects. Failure to obtain the hypothesized increased water consumption is attributed to the competition of anxiety with the previously established consummatory responses.

Although the data presented by Kendler & Law seem to be in opposition to those of Amsel and Amsel & Maltzman, it should be noted that there were wide differences in the experimental situations. It is possible that no broad generalizations can be established to describe adequately the effect of drive interaction.

Partial reinforcement.—Ever since the demonstration of the Humphreys effect, the problem of the resistance to extinction of a partially reinforced response has attracted attention. As in the case of latent learning, the interest stems, at least in part, from the fact that the phenomenon was first thought to offer a critical test of S-R as opposed to S-S theory. Even though it is now apparent that there are probably no critical tests of present-day learning theories, the Humphreys effect is an extremely interesting and important phenomenon—a phenomenon that appears to be rather widely generalized and of practical significance.

Linton & Miller (60) investigated the effect of partial versus 100 per cent reinforcement in a straightaway alley on subsequent conditions of extinction (hunger, no food incentive) and satiation (no hunger, food incentive). Comparison of the four groups showed that the partially reinforced extinction group was significantly more resistant to extinction than its consistently reinforced control; the partially reinforced satiation group maintained the

XUM

originally learned performance more effectively than did its control, but the differences were not statistically significant. Interpretation is made in terms of frequent reinforcement of the cues produced in the preceding nonreinforced trials.

Comparison of the extinction of the learned responses of escape and avoidance of shock from the grilled-floor half of an enclosed runway was made by Sheffield & Temmer (95). As measured by speed of locomotion, the rats in the avoidance group were poorer in performance but more resistant to extinction than the escape animals during most of training. The results are interpreted in terms of the partial reinforcement of the avoidance animals and the 100 per cent reinforcement of the escape animals during training, so that avoidance training resulted in "conditioning the instrumental response to cues characteristic to a series of nonreinforced trials" (95, p. 297).

Crum, Brown & Bitterman (23) question the standardized S-R interpretation of the effect of partial and delayed reinforcement, inasmuch as they found no differences in extinction between a group of rats given 50 per cent reinforcement and a second experimental group given 100 per cent reinforcement but with reinforcement delayed 30 sec. on half of these trials.

Grant, Hornseth & Hake (34) found that the intertrial interval had no effect on the acquisition or extinction of a randomly reinforced verbal response. They conclude (34, p. 612):

> That a general theoretical treatment of partial reinforcement phenomena is premature is illustrated by the fact that the present findings differ markedly from those of Sheffield on a different response in a different species.

In a typical nonspatial discrimination problem the positive one of the relevant cues [as circle (+), triangle (−)] is given 100 per cent reinforcement, and differential irrelevant cues (as right position, left position) are given equal partial reinforcement (i.e., each 50 per cent). Bitterman & Coate (12), for purposes subsequently described, utilized a technique of biased partial reinforcement (80 per cent, 20 per cent) of the differential irrelevant positional cues. The general technique of biased partial reinforcement of differential irrelevant cues is capable of many extensions and variations that should make possible new and penetrating analyses of a wide range of learning problems.

Secondary reinforcement.—It should be obvious that the phenomenon of secondary reinforcement will of necessity be of utmost importance in the formulation of any comprehensive behavior theory. As we ascend the phyletic scale, a progressively greater proportion of the animals' behavior appears to depend on mechanisms other than those of primary reinforcement. Although this important aspect of the problem of secondary reinforcement has received scant attention, the phenomenon of secondary reinforcement has been well established and adequately described.

Spence's suggestion (103a) that there may be no primary delay-of-reinforcement gradient and that all learning with delay involves the develop-

ment and operation of immediate secondary reinforcement emphasizes the importance of determining under varied conditions the limits and forms of delay-of-reinforcement gradients. Smith (101) determined a visual stimulus-trace gradient in 49 rats by interposing a delay chamber between the choice point of a T-maze and the white or black straightaway which served as a cue for a right or left turn. A hyperbolic gradient of learning as a function of the delay interval was obtained, and it was essentially asymptotic at 5 sec.

Two experiments on secondary reinforcement are reported by Bersh (10). One varied the duration of a neutral stimulus from 0 to 10 sec. prior to the presentation of food and determined the effect of such variation upon the acquisition by the neutral stimulus of power to reinforce the previously untrained operant response of bar-pressing. A maximum was obtained in the range between 0 and 2 sec. The curve had not attained its asymptote at 10 sec. The second experiment measured the effect of the number of pairings of the neutral stimulus and the reinforcing agent upon the subsequent reinforcing power of the neutral stimulus. Significant differences were obtained between the 120-pairing group and the 0- and 20-pairing groups.

A test of the power of secondary reinforcing agents to reduce the strength of a primary drive was made by Simon, Wickens, Brown & Pennock (99). They found that the secondary reinforcing stimuli (buzzer and light) in the absence of the primary reinforcing stimulus (water) had no effect on the amount of water subsequently drunk by rats. Thus, no evidence was obtained for the hypothesis that secondary reinforcing agents temporarily acquire physiological equivalence to the primary reinforcing agent.

Incentives and learning.—Reinforcement theory has tended to emphasize the importance of drive state and has given relatively little emphasis to the role of incentives in determining habit formation or performance measures. At the present time, however, there is evidence indicating the importance of incentive variables and suggesting that drive-state variables are unimportant or of only secondary importance in determining efficiency of much learning and learned performance. Such a point of view is not to be taken to mean that any of the experiments cited is a critical test of any theory. Undoubtedly, all the experiments in this section can be described in terms of S-R theory, a type of theory which has shown remarkable ability to keep almost abreast with fact.

Using monkeys as subjects, Meyer (68) investigated the effect of large variations in time of maintenance-food (Purina Chow, carrots, and oranges) deprivation upon efficiency of learning rewarded by test-food (peanuts and raisins) incentives. Deprivation periods of 1, 23, and 48 hr. failed to produce differences in any of various measures of learning of discrimination-reversal problems. Similar results were obtained on a series of discrimination learning problems presented 1.5, 6.5, and 22.5 hr. after maintenance-diet feeding.

A novel variation of the sham-feeding experiments is presented by Sheffield & Roby (94) in a study of the reward value of a sweet taste produced

XUM

by saccharine, a nonnutritive substance. Both hungry rats and satiated rats rapidly learned to take almost all their water from a bottle containing saccharine solution in preference to a bottle containing water, and both groups markedly increased their total fluid intake. The same hungry rats learned over a period of 18 days to approach and drink from a saccharine solution bottle when it was inserted in a cage that always contained a bottle of plain water. Satiated rats, however, showed no learning in this situation. Finally, the group of hungry rats was successfully trained to go to the saccharine-rewarded arm in preference to the water-rewarded arm of a single-unit T-maze. Sheffield & Roby discount the possibility that the sweet taste was an acquired reward and suggest that "eliciting the consummatory response of ingestion was the critical factor" in the power of the saccharine solution to serve as a reinforcing agent in the absence of primary drive satisfaction (94, p. 481).

An equally ingenious experiment by Sheffield, Wulff & Backer (96) was designed to test the reinforcing role in male rats of sexual stimulation in the absence of drive reduction through ejaculation. The learning measure was running speed down a runway. Males which had initiated copulation in preliminary tests were divided into an experimental group which ran to a female lure and were permitted intromission but not ejaculation and a control group which ran to males. A third group was composed of those males which had not copulated in preliminary tests. The experimental group ran faster than the other groups. Attempted copulation, not necessarily intromission, appeared to be the critical reinforcing factor. The authors appropriately conclude (p. 8):

> On the whole, the present findings emphasize the incentive value of rewards—rather than their satisfying or relaxing value—as a critical factor in selective learning.

Theoreticians may have difficulty, however, in seeing why these data best fit (p. 7)

> the contiguity theory of Guthrie, which regards the ability of goal objects to take the animal out of the prior situation as the significant aspect of reinforcing events.

Learning motivated by a manipulation drive, observed by Harlow (40) in monkeys, also appears to depend upon the incentive value of the manipulanda, since it is difficult to believe that any physiological drive is reduced by this process. Buxton's suggestion (19) that the manipulation drive proposed in a previous study (43) might have a history of acquisition via reward over the animal's pre-experimental lifetime is contraindicated by the growth in strength of the response with numerous successive, successful nonrewarded trials. As suggested in the earlier paper (43), if secondary motivation were the basis of the learning, loss of response strength, not gain, would be expected.

It is likely that, regardless of any other trends in learning experimentation, the investigation of the role of incentives in learning will indirectly

lead to additional emphasis on analysis of more and more complex forms of learning. There is reason to believe that the presence of intense internal drives may facilitate simple forms of acquisition such as bar-pressing and single-unit T-maze discrimination but seriously impair, if not completely inhibit, learning of problems of greater complexity. The incentive experiments have for the most part been formulated outside the reinforcement mold. There is every reason to believe that as incentive data accumulate, they will change and doubtless redirect emphasis within learning theory. Some psychologists, in personal communications to the reviewer, have expressed the opinion that we are returning to psychological hedonism—described, of course, in objective terminology.

Temporal and Perceptual Factors in Learning

Many factors other than the motivational factors of drive state and incentive condition act to influence learning and learned performance. A very considerable interest has been shown during the past year in the analysis of some of these factors.

Temporal factors.—The temporal relationships between stimuli have been subjected to intensive analysis in the past, and many of the basic problems have been clarified. The technical problems of simultaneous and backward conditioning and distribution of practice, however, continue to attract interest.

Representative of the temporal relationship studies of the past year is an experiment by Cohen (22), who investigated the formation of a withdrawal response based on strictly simultaneous presentation of a click and foreleg shock. Conditioned responses typically appeared early in training in the three subjects (two goats and a sheep), quickly reached maximum strength, then underwent extinction. That extinction should occur is not surprising inasmuch as the test for the conditioned response (click alone) is itself an extinction trial and differs basically from the training stimulus pattern of click plus shock. There is even some question that the click played any role in the learned response; it is reported that at least in one subject during the period of training in which the withdrawal response occurred there was evidence of sensitization to extraneous stimuli.

The effect of the temporal position of a rotated maze interpolated between learning and relearning of the original maze was investigated by Langhorne (57) in human subjects. No differences were found in the relearning scores of groups of subjects given the rotated maze immediately after learning, just before relearning, or at the midpoint (24 hr.) between learning and relearning.

Perceptual information.—Evidence continues to amass indicating that efficiency of learning is greatly influenced by the amount of perceptual information given the animal and that training is an extremely important factor determining the perceptibility of stimuli or categories of stimuli for animals ranging at least from rat to man.

XUM

Saldanha & Bitterman (87) present a new technique which utilizes combinations of two pairs of stimuli differing in one dimension each. In the second experiment, the A (comparison) group of rats learned concurrently a brightness discrimination and a striped-pattern discrimination. The B (noncomparison) group also learned two discriminations concurrently, but each of its stimulus pairs contained one of the gray and one of the striped stimuli. Following initial training, the A group was shifted to the B problems and the B group to the A problems. The comparison group was more efficient than the noncomparison group. In the first experiment, which utilized other stimuli but followed the same general design, the differences between the groups were not significant. The experimenters interpret the results of the second experiment as a confirmation of a theory of relationally determined perception and learning and a refutation of Spence's stimulus-response theory of discrimination learning. Whereas the results permit a relational interpretation, it is well to point out that they do not rule out a stimulus-response interpretation. It is unlikely that Spence would accept the study as a test of his theory inasmuch as the experimental conditions differ markedly from those his theory was designed to meet.

Human subjects were employed by Saul & Osgood (89) in an experiment which failed to yield evidence that perceptual organization, measured by isolating as opposed to massing of items in serial discrimination lists, influenced learning or retention.

Reactive Inhibition

Hull's construct of reactive inhibition suggests a mechanism which could account for some of the variability of behavior so characteristic of animals. Intensive studies of this construct have been reported during the past year.

A test of reactive inhibition was made by Siegel (98), who familiarized 250 human subjects with a situation in which either of two switches turned off a light. He instructed groups of students to make 0, 20, 40, 80, or 160 responses to the right-hand switch, and then permitted free choice on 10 trials. A rectilinear relationship obtained between frequency of response to the left-hand switch in the free-choice situation and frequency of response to the right switch in the preceding forced-choice phase. Siegel attributes the results to the operation of reactive inhibition.

The result of reinforcing responses in a coin-guessing experiment conducted by Bendig (9) was to decrease the number of alternations of response, a result in keeping with Hull's learning theory. The investigator clearly points out, however, that individual concepts of probability may be more important in human guessing behavior than concepts of reinforcement and reactive inhibition.

The effect of three conditions of work effort and three different intertrial intervals on the latency of a bar-pressing response during the course of both acquisition and extinction was tested by Montgomery (71). During

acquisition, mean response time was positively related to work and negatively related to intertrial interval. Work load facilitated extinction, but intertrial interval gave somewhat variable results. Three of eight predictions based on Hull's theory were confirmed, and theoretical explanations are offered to account for the negative findings.

That there is no simple relationship between work effort and efficiency of discrimination learning in monkeys is clearly apparent from the studies of Davis cited by Harlow (42, pp. 214–15). A high degree of effort gave maximally efficient performance, and a low degree of effort, relatively inefficient performance.

Extinction

Extinction remains a key conditioning phenomenon and, as such, has stimulated work from investigators of varying theoretical interests. The role of operant level in conditioning and extinction of a bar-pressing response was measured by Bullock (17), who found significant positive correlations between operant level and reserve, and between the operant rate and extinction ratio combined and reserve. Extinction ratio and reserve were also significantly correlated, but a low, insignificant positive correlation was obtained between operant rate and extinction ratio.

Dinsmoor (27) trained rats for 200 cycles to make a discriminative bar-pressing response, and then he extinguished this response under three different conditions: (*a*) in the presence of S^D but with each response followed by 3 sec. of S^Δ; (*b*) in the presence of S^Δ with each response followed by 3 sec. of S^D; and (*c*) without S^D at any time. No significant differences were found between the first two (experimental) groups, but both differed significantly from the control group. Reversal of the extinction technique in half of each experimental group during the second half of the extinction series produced no significant differences.

Sheffield (97) trained 72 rats under either massed- or spaced-training conditions to run down a 4-ft. alley for food, and subsequently she subdivided the groups and extinguished half of each training group under massed trials and half of each training group under spaced trials. Spaced extinction was significantly faster for the spaced-training group and somewhat faster for the massed-training group. She accounts for the results by assuming th[illegible] the frustration of nonreinforcement produces increased vigor of response and that this frustration under massed extinction has less opportunity to become dissipated than under spaced extinction.

The effect of massing and spacing of trials on the extinction of pecking responses in pigeons reinforced for 200 trials under either continuous or aperiodic conditions was tested by Jenkins, McFann & Clayton (51). The aperiodically reinforced birds elicited almost four times as many extinction responses as did the continuously reinforced subjects. No significant differences were found relating to massing or spacing of the extinction trials.

The effect of three different extinction conditions upon subsequent

XUM

reconditioning in the dog was measured by Deese (26). The conditions were simple extinction, generalized extinction to a tone different from the conditioning tone, and extinction concurrent with acquisition of a response of the contralateral hind leg. The greatest decrement as measured by reconditioning obtained in the generalized extinction group, and the least decrement obtained in the group in which a new conditioned response was formed concurrently with extinction of the old.

Discrimination and Discrimination Reversal

Steady progress has been made in the analysis of discrimination learning and the learning of partial or complete discrimination reversals. Much of this work has now transcended the original scope of the continuity-discontinuity controversy.

Continuity in learning by naive animals gains additional support. Ritchie, Ebeling & Roth (86) tested 20 rats on a horizontal pattern versus a vertical pattern, using a Lashley jumping stand and a Spence-type five-period procedure. The results throughout the learning period support a continuity theory.

A second experiment also in accord with continuity theory was carried out by Grice (36) as a test of a technique designed by Lashley & Wade (58). Two groups of 10 rats were trained to respond to a black square opposed to either a 5- or 8-cm. circle. The two groups were subsequently trained on the two circular stimuli, and the group whose combined training involved reversal was less efficient.

Using an original technique of biased reinforcement of the irrelevant cues during black-white discrimination learning and during the evocation trials of an already learned positional discrimination, Bitterman & Coate (12) demonstrate that the differentially reinforced irrelevant cues acquire functional significance under conditions which do not permit the consistent confirmation of "hypotheses" about them. These results provide support for the continuity learning theory of initial-problem learning by naive animals.

Two detailed analytical studies are presented by North (80, 81), who clearly demonstrates improvement in rats on successive discrimination reversals of a spatial discrimination problem. Three major variables—amount of practice between reversals, distribution of practice, and postchoice conditions pertaining to error correction—were measured in a factorial-design experiment. Improvement in reversal performance occurred under all conditions, but in none, contrary to an earlier report by Buytendijk (20), was one-trial reversal attained. Interpretation of the limited interproblem improvement is given in terms of habit dominance, perseverative-cue theory, and "acquired distinctiveness of cues"; the operation of none of these explanatory factors is ruled out.

The relatively inefficient performance of rats during an extended series of simple spatial discrimination reversals contrasts with the extremely efficient

performance of monkeys on more complex, nonspatial discrimination reversals, as reported by Harlow (39). After formation of appropriate learning sets there is unquestionable discontinuity in the intraproblem learning of both discriminations and discrimination reversals. The learning sets appear to develop slowly and continuously, and improvement parallels the gradual reduction of various error-producing factors.

A theory integrating the intraproblem and interproblem data of discrimination learning and discrimination-reversal learning has been proposed by Meyer in two papers (68, 69). As learning sets form, there is progressively increasing discontinuity between the learning curve segment composed of the first and second trials and the segment made up by the remaining trials. The first segment is described as the fast-rate portion and the second as the slow-rate portion. The proposal is made that the slow-rate functions of all intraproblem reversal learning curves can be described by a single general equation giving a function of constant-decay form. As practice proceeds on successive problems, the slow-rate functions are initiated nearer to the asymptote, with the result that (68, p. 15)

> the steep functions obtained in the earlier problems and the flat functions that appear in the late problems are the rapid and asymptotic portions of a single nonlinear curve.

This extremely ingenious hypothesis describes and relates to a single function the continuous and orderly changes from the continuity learning of an initial discrimination problem to the discontinuity of subsequent discrimination problems.

Generalization and Transposition

An interesting technique for obtaining gradients of generalization of learned verbal responses has been reported by Underwood & Hughes (115), who measured the learning and retention of a list of paired nonsense syllables and adjectives. The errors were carefully scaled along the four dimensions of synonymity, antonymity, homonymity, and contextual similarity. Analyses for the first three dimensions revealed reliable evidence for gradients of generalized verbal responses—the greater the similarity between the errors and the correct responses, the more frequent the errors.

New evidence favorable to Thorndike's spread-of-effect phenomenon is presented by Marx & Bunch (65). Two new gradients, the interstimulus error-recovery gradient and the intrastimulus error-recovery gradient, are described, and evidence is presented arguing against the explanation of these gradients as artifacts resulting from number-guessing habits or probability-bias factors—the common criticisms made against the Thorndike gradient. A tentative hypothesis (p. 104) offered to explain the gradients assumes "that responses are organized into a variety of functional systems which are susceptible to interaction effects resulting from reinforcement."

Reward by saying "right" was found by Duncan (28) to increase response repetition and to reduce response variability, but no evidence for spread

of effect was found. No response, a meaningless aftereffect, and punishment by saying "wrong" produced no effect whatsoever.

A novel technique for measuring generalization of voluntary responses to visual stimuli is reported by Brown, Bilodeau & Baron (15), who instructed their subjects to respond by lifting their fingers from a reaction key only when the middle lamp in a horizontal row of seven lamps was lighted. With frequency of response as the measure, a symmetrical gradient from the central to the peripheral lights was obtained.

Alberts & Ehrenfreund (1) demonstrated a progressively decreasing transposition gradient to a learned size discrimination for children under four years of age and almost perfect transposition at all test points for older children. The study extends and confirms the earlier experiment of Kuenne (56).

Grice & Saltz (37) present data showing size generalization in rats trained under conditions of successive stimulus presentation. Generalization functions were obtained to stimuli both larger and smaller than the training stimulus, and the data are interpreted in terms of Hull's "stimulus intensity dynamism." Of five predictions based on Hull's theory, one prediction is verified, another "essentially verified," a third "partially verified," and possible explanations for all discrepancies are presented.

Transposition of learned brightness discrimination by rats was tested by Kendler (54). Rats trained to choose the brighter of two stimuli showed decreasing transposition for three steps but a reversal for steps 4 and 5. The converse training condition gave decreasing transposition for three steps and no change on the fourth (final) step. The author correctly states (54, p. 561):

> These data do not appear to corroborate all the expectations from the set of theoretical generalization curves utilized by Spence to illustrate his theory.

Verbal Learning

Experiments involving verbal learning have already been presented under various subheadings. A number of important contributions, however, do not readily fall under the previous classifications and are accordingly presented here.

Morgan & Underwood (73) tested 24 subjects by the anticipation method on lists of paired two-syllable adjectives. The effect of synonymity of the response items in the two lists was measured by systematic variation of five experimental conditions. Associative facilitation of second-list learning was positively related to response synonymity. Proactive inhibition of recall was inversely related to response similarity, but significant differences were obtained only under the condition of maximal response dissimilarity. Associative-facilitation results are explained in terms of response generalizations.

The phenomenon of skewness in the bowed serial-position curve led Ribback & Underwood (84) to append or prefix a new syllable to each pair

of a previously learned list of paired nonsense syllables. The appended-syllable list was learned more rapidly than the prefixed-syllable list. The data are submitted to account, at an empirical level, for the asymmetry of the serial-position curve.

Greenberg & Underwood (35) tested 72 college students on the learning, recall, and relearning of four lists of paired adjectives. The interval between learning and recall was 10 min. for one group, 5 hr. for a second, and 48 hr. for a third. Each group learned, recalled, and relearned four lists. Marked positive transfer was obtained from learning the first to learning the second list, but little additional improvement appeared in learning the third and fourth lists. Stage of practice was unrelated to recall or relearning when a 10-min. retention interval was employed. With the 5-hr. and 48-hr. intervals, there was an inverse relation between stage of practice and recall, but no correlation between stage of practice and relearning.

Evidence that a "warm-up effect" influences rate of acquisition is presented independently by Thune (111) and by Hamilton (38). Thune found that efficiency of performance on a 10-trial list of paired-associate nouns was directly related to the number of warm-up trials, from 0 to 10, given 10 min. previously. Warm-up practice on a nonassociative color-guessing task proved equally efficient in facilitating the subsequent paired-associates test list. The facilitating "warm-up effect" was reported by Hamilton (38) to decrease as a negatively accelerated function from 8 sec. to 1 hr. and then to remain essentially constant as measured by performance on appropriate lists of paired associates.

Noble (79) failed to confirm the Ward-Hovland reminiscence effect either following one presentation of 12 trisyllabic adjectives or after learning had been carried to a criterion of anticipation of seven words in a list. No degree of prior learning, ranging from zero degree to perfect list recitation, was found by Underwood (114) to result in any proactive inhibition when 8 sec., instead of 2 sec., were allowed for recall of each item. The longer recall time increased the absolute number of interlist intrusions at recall, but not the proportion of intrusions as a function of degree of prior learning. The data of this and earlier experiments provide evidence in support of Melton's unlearning theory.

Postman (82) tested subjects on a list of 36 nonsense syllables and measured recognition memory when one, three, six, and nine incorrect alternatives were offered. Recognition memory declined gradually until nine incorrect choices were presented, and then it dropped sharply.

Motor Skills Learning

Increased interest in motor skills learning and retention has been apparent in the last few years and, in part, stems from, and is dependent upon, the important role that this type of learning plays in military and industrial situations.

Positive transfer of both color discrimination and position discrimination

XUM

to a motor task involving both of these components is reported by Gagné, Baker & Foster (31). The more difficult problem, the color discrimination, subsequently transferred to both the color and position components of the motor task, whereas the less difficult position discrimination transferred only to the position component. The following generalization is presented (p. 325):

> . . . preliminary training on a given task reduces the final-task errors associated with discriminations equally or less difficult, but does not reduce those errors associated with a more difficult discrimination.

Stimulus predifferentiation involving varying amounts of learning of letter "names" to four different light stimuli facilitated discriminative motor skills learning to the four light-stimulus cues. Significant differences were obtained by Gagné & Baker (32) only after rather extended stimulus predifferentiation practice. Essentially similar results were found by Baker & Wylie (6) when pretraining for the motor task was given in the form of learned verbal relationships with knowledge by the subject that this training would be transferred to a motor task.

Baker, Wylie & Gagné (7) tested the transfer effect of learning at a particular rate a crank-turning task which aligned a pointer with an irregularly moving target. Relative amount of transfer of rate of responding was positively related to the degree of similarity of the training and the final task.

Seven groups of subjects were trained by Ammons (2) on a rotary-pursuit task with intertrial intervals ranging from 0 to 24 hr. All groups were given two subsequent massed practice periods, and the most efficient performance on these tests was made by the subjects with intermediate-length rest periods between training trials. Ammons (3) has also presented a detailed analysis of individual records of rotary-pursuit performance under massed and spaced practice conditions. The number of hits rises rapidly under distributed practice and slowly under continuous practice, and mean duration of hits shows a negatively accelerated increase during practice under both conditions.

Spaced practice was found by Riopelle (85) to be superior to massed practice in learning on the Vector Complex Reaction Time Test, and the differences between the two groups increased as learning progressed. Size of target and speed of rotation were found by Helmick (44) to be factors influencing efficiency of performance on a Koerth-type pursuit rotor test.

Two studies bearing on Snoddy's theory of primary and secondary growth were conducted by Tsao. In the first experiment (112) early massing and later spacing of mirror-drawing practice trials was at least as efficient as early spacing and later massing. In the second experiment (113) there was progressive improvement from trial to trial when six sessions of two continuous trials each were run at 12-hr. intervals. The results of neither experiment are in keeping with Snoddy's theory.

XUM

Complex Learning, Reasoning, and Thinking

A qualitative theoretical position has been presented by Harlow (41) designed to interrelate learning and thinking problems in a continuum in terms of the complexity and multiplicity of the factors producing difficulty. Similar formulations arguing for continuous transition from simple association through concept formation, symbolization, and cognition have been advanced by Nissen (78) and by Leeper (59). A review of the literature on concept formation in children and adults has been presented by Vinacke (116), who also describes the psychologists' achievements to date and the unfulfilled goals in this area.

Baker (8) has reported solution by 10 rats of a problem which may be regarded as a simple conditional or multiple-sign problem. The subjects were trained to choose the lighted alley of a modified Yerkes-Watson discrimination box on food-deprived days and the dark alley on water-deprived days. Critical tests showed that the efficient conditional response was based on the cues derived from drive condition and not temporal alternation. Since the second-order cues afforded by drive state were only shifted on alternate days and after five trial-blocks, effective conditions existed for holding interference between the second-order cues to a minimum.

An extremely complex conditional reaction in the chimpanzee was described by Nissen (77), who tested a single animal for a three-year period. Solution of the final steps of the problem involved simultaneous responsiveness to five different cues, and the subject averaged 83 per cent correct responses during the final 15 sessions.

A variation of the original Tolman & Honzik inferential expectation apparatus, which provided "every essential condition specified by Tolman and Honzik as necessary for the development of 'insight' behavior on the test runs" (16, p. 229) failed to yield any evidence of "insight" in rats. As Brown & Gentry (16) point out, inasmuch as all other investigators subsequent to Tolman & Honzik have also failed to demonstrate "insight," the conclusions of the pioneer investigators are open to re-evaluation and simpler interpretation.

A study in productive thinking in human subjects was carried out by Birch & Rabinowitz (11), who tested three groups of students with different experimentally produced past experiences on the Maier two-cord problem. The subjects gave evidence of Duncker's "functional fixedness" in that they tended to avoid use of the particular object with which they had had previous experience as, e.g., the pendulum weight for solution of the Maier problem.

Marks (64) reports a detailed study of the problem-solving ability of 82 college students who were taught to compute square roots with a calculating machine and an auxiliary table of factors. The problem in the personal situation involved errors actually made by the subject and resulting from the experimenter's falsifying the table. In the impersonal situation, the subject was asked to solve the problem of a mythical character presented in a written

description of the personal situation. Superior performances were obtained in the impersonal situation. The use of aids in the form of Socratic questioning also improved performance. Training in the form of a 1-hr. lecture given three weeks previously produced no significant effect.

In a concept formation study by Buss (18) separate groups of subjects were trained on five learning series, each having a different proportion of tall blocks (positive instances) and short blocks (negative instances), and all groups were then given an identical criterion series. The author concludes (p. 500) that

> the more stimuli of one kind presented to the subject in the learning series, the greater is the tendency to categorize these stimuli correctly.

Where all instances were either positive or negative, differentiation between the two size categories was discovered to be poor. The instructions preceding training were held constant for all five groups but were such that in practice they were factual for three groups and fallacious for two.

A further study of the Weigl-type card sorting tests by Grant (33) revealed that sorting for number was more difficult when the configuration of forms differed than when it was held constant and that the difference was most marked when the subjects were first confronted with number sorting.

Dattman & Israel (24) tested difficulty in the acquisition of concepts of concrete objects, forms, and numbers with the Heidbreder materials so altered as to supply equivalent perceptual instances. No differences were found among the three categories, and it was concluded that the relative ease with which any category of concepts is attained is directly dependent upon the perceptual effectiveness of the instances in presenting the features to be conceptualized.

Reed (83) has continued his researches on concept formation with a study of the comparative economy of serial versus simultaneous presentation of varying lengths of series. Simultaneous presentation proved to be far more accurate than serial presentation for 24-card series, but the differences approach zero as the length of the series is increased. The serial method was significantly more effective than the simultaneous in terms of number of promptings required to form concepts on the 24-card series, but these differences approached zero as the length of series was increased to 42 or 60 cards.

The learning of multiple-alternation problems in the white rat was studied by Woodbury (119), using a five-, seven-, or nine-unit linear maze with two parallel doors at each choice point. Solution of these problems was attained within 50 trials by 50, 37, and 17 per cent of the subjects in the double-, triple-, and quadruple-alternation groups, respectively. These results are far superior to those obtained by most earlier investigators. In all cases the greatest difficulty was found at the choice just before the directional reversal, and there was a convex gradient of frequency of correct choices extending in both directions from this point. In a theoretical article the writer explores "the possibilities of rigorous derivation of certain of the results of this experiment from hypotheses advanced by Hull" (120, p. 18),

and shows that Hull's theory provides all the assumptions necessary. Woodbury makes no claim that his results would be predicted from Hull's theory and recognizes that many of his essential data are the inverse of those found by other investigators of multiple alternation using other apparatus and other animals.

Frick & Miller (30) analyzed and statistically described the behavior of rats in a modified Skinner box with food tray and bar at opposite ends and a holding relay operating to present food only for the first of a series of bar responses. Operant level, conditioning, and extinction behavior were successively measured. Analysis was made in terms of patterns of response, and the prediction of the temporal order of the rats' behavior was found to be more uncertain after training than before, since training "caused the animal to abandon one well organized pattern of behavior for another" (p. 25). A graph for portraying simple courses of action was constructed, and the individual courses of action under the qualitatively different conditions of operant level, deprivation, reinforcement, and extinction are quantitatively expressed in terms of the co-ordinates of the graph. This extremely important paper is admittedly a methodological study designed to gain experience in the application of the rather difficult mathematical techniques employed in modern communication theory to such "challenging targets" as human verbal behavior.

Conclusion

The psychological researches in the broad field of learning continue the trend apparent in the last few years of being oriented in terms of various broad theoretical systems. This holds particularly for studies carried out in the area of psychological rodentology, but increasingly frequent attempts are being made to extend learning theory to researches with human subjects in the areas of motor skills learning, verbal learning, and complex learning–thinking activities.

XUM

LITERATURE CITED

1. Alberts, E., and Ehrenfreund, D., *J. Exptl. Psychol.*, **41**, 30–38 (1951)
2. Ammons, R. B., *J. Exptl. Psychol.*, **40**, 777–87 (1950)
3. Ammons, R. B., *J. Exptl. Psychol.*, **41**, 17–22 (1951)
4. Amsel, A., *J. Exptl. Psychol.*, **40**, 709–15 (1950)
5. Amsel, A., and Maltzman, I., *J. Exptl. Psychol.*, **40**, 563–69 (1950)
6. Baker, K. E., and Wylie, R. C., *J. Exptl. Psychol.*, **40**, 632–38 (1950)
7. Baker, K. E., Wylie, R. C., and Gagné, R. M., *J. Exptl. Psychol.*, **40**, 721–32 (1950)
8. Baker, R. A., *J. Comp. Physiol. Psychol.*, **43**, 409–15 (1950)
9. Bendig, A. W., *J. Exptl. Psychol.*, **41**, 105–7 (1951)
10. Bersh, P. J., *J. Exptl. Psychol.*, **41**, 62–73 (1951)
11. Birch, H. G., and Rabinowitz, H. S., *J. Exptl. Psychol.*, **41**, 121–25 (1951)
12. Bitterman, M. E., and Coate, W. B., *J. Comp. Physiol. Psychol.*, **43**, 198–210 (1950)
13. Brogden, W. J., *Psychol. Rev.*, **58**, 224–29 (1951)
14. Brogden, W. J., in *Handbook of Experimental Psychology*, Chap. 16, 568–612 (Stevens, S. S., Ed., John Wiley & Sons, Inc., New York, N. Y., 1436 pp., 1951)
15. Brown, J. S., Bilodeau, E. A., and Baron, M. R., *J. Exptl. Psychol.*, **41**, 52–61 (1951)
16. Brown, W. L., and Gentry, G. V., *J. Comp. Physiol. Psychol.*, **43**, 226–30 (1950)
17. Bullock, D. H., *J. Exptl. Psychol.*, **40**, 802–4 (1950)
18. Buss, A. H., *J. Exptl. Psychol.*, **40**, 494–503 (1950)
19. Buxton, C. E., *Ann. Rev. Psychol.*, **2**, 223–44 (1951)
20. Buytendijk, F. J. J., *Arch. nëerland. physiol.*, **15**, 283–310 (1930)
21. Caldwell, W. E., *J. Genetic Psychol.*, **78**, 3–38 (1951)
22. Cohen, J., *J. Comp. Physiol. Psychol.*, **43**, 211–16 (1950)
23. Crum, J., Brown, W. L., and Bitterman, M. E., *Am. J. Psychol.*, **64**, 228–37 (1951)
24. Dattman, P. E., and Israel, H. E., *J. Psychol.*, **31**, 147–60 (1951)
25. Deese, J., *Psychol. Rev.*, **57**, 291–94 (1950)
26. Deese, J., *J. Exptl. Psychol.*, **40**, 488–93 (1950)
27. Dinsmoor, J. A., *J. Exptl. Psychol.*, **40**, 458–72 (1950)
28. Duncan, C. P., *J. Exptl. Psychol.*, **40**, 380–89 (1950)
29. Finan, J. L., *J. Comp. Psychol.*, **29**, 119–34 (1940)
30. Frick, F. C., and Miller, G. A., *Am. J. Psychol.*, **64**, 20–36 (1951)
31. Gagné, R. M., Baker, K. E., and Foster, H., *J. Exptl. Psychol.*, **40**, 314–28 (1950)
32. Gagné, R. M., and Baker, K. E., *J. Exptl. Psychol.*, **40**, 439–51 (1950)
33. Grant, D. A., *J. Exptl. Psychol.*, **41**, 23–29 (1951)
34. Grant, D. A., Hornseth, J. P., and Hake, H. W., *J. Exptl. Psychol.*, **40**, 609–12 (1950)
35. Greenberg, R., and Underwood, B. J., *J. Exptl. Psychol.*, **40**, 452–57 (1950)
36. Grice, G. R., *J. Comp. Physiol. Psychol.*, **44**, 149–53 (1951)
37. Grice, G. R., and Saltz, E., *J. Exptl. Psychol.*, **40**, 702–8 (1950)
38. Hamilton, C. E., *J. Exptl. Psychol.*, **40**, 613–21 (1950)
39. Harlow, H. F., *J. Comp. Physiol. Psychol.*, **43**, 231–39 (1950)

40. Harlow, H. F., *J. Comp. Physiol. Psychol.*, **43,** 289–94 (1950)
41. Harlow, H. F., in *Theoretical Foundations in Psychology,* Chap. 10, 452–505 (Helson, H., Ed., D. Van Nostrand Co., Inc., New York, N. Y., 787 pp., 1951)
42. Harlow, H. F., in *Comparative Psychology,* Chap. 7, 183–238 (Stone, C. P., Ed., Prentice-Hall, Inc., New York, N. Y., 525 pp., 1951)
43. Harlow, H. F., Harlow, M. K., and Meyer, D. R., *J. Exptl. Psychol.*, **40,** 228–34 (1950)
44. Helmick, J. S., *J. Exptl. Psychol.*, **41,** 126–38 (1951)
45. Heron, W. T., in *Comparative Psychology,* Chap. 6, 137–82 (Stone, C. P., Ed., Prentice-Hall, Inc., New York, N. Y., 525 pp., 1951)
46. Hilgard, E. R., in *Handbook of Experimental Psychology,* Chap. 15, 517–67 (Stevens, S. S., Ed., John Wiley & Sons, New York, N. Y., 1436 pp., 1951)
47. Horenstein, B. R., *J. Comp. Physiol. Psychol.*, **44,** 210–24 (1951)
48. Hovland, C. I., in *Handbook of Experimental Psychology,* Chap. 17, 613–89 (Stevens, S. S., Ed., John Wiley & Sons, New York, N. Y., 1436 pp., 1951)
49. Hull, C. L., *Principles of Behavior* (Appleton-Century-Crofts, Inc., New York, N. Y., 422 pp., 1943)
50. Hull, C. L., *Psychol. Rev.*, **57,** 303–13 (1950)
51. Jenkins, W. O., McFann, H., and Clayton, F. L., *J. Comp. Physiol. Psychol.*, **43,** 155–67 (1950)
52. Kendler, H. H., and Kanner, J. H., *J. Exptl. Psychol.*, **40,** 762–65 (1950)
53. Kendler, H. H., and Law, F. E., *J. Exptl. Psychol.*, **40,** 299–304 (1950)
54. Kendler, T. S., *J. Exptl. Psychol.*, **40,** 552–62 (1950)
55. Koch, S., and Daniel, W. J., *J. Exptl. Psychol.*, **35,** 162–85 (1945)
56. Kuenne, M. R., *J. Exptl. Psychol.*, **36,** 471–90 (1946)
57. Langhorne, M. C., *J. Gen. Psychol.*, **43,** 283–94 (1950)
58. Lashley, K. S., and Wade, M., *Psychol. Rev.*, **53,** 72–87 (1946)
59. Leeper, R., in *Handbook of Experimental Psychology,* Chap. 19, 730–57 (Stevens, S. S., Ed., John Wiley & Sons, New York, N. Y., 1436 pp., 1951)
60. Linton, H. B., and Miller, N. E., *J. Comp. Physiol. Psychol.*, **44,** 142–48 (1951)
61. London, I. D., *Psychol. Rev.*, **57,** 295–302 (1950)
62. London, I. D., *Psychol. Rev.*, **58,** 54–59 (1951)
63. Maltzman, I. M., *J. Exptl. Psychol.*, **40,** 788–93 (1950)
64. Marks, M. R., *J. Exptl. Psychol.*, **41,** 74–80 (1951)
65. Marx, M. H., and Bunch, M. E., *J. Exptl. Psychol.*, **41,** 93–104 (1951)
66. Meehl, P. E., and MacCorquodale, K., *J. Comp. Physiol. Psychol.*, **44,** 178–83 (1951)
67. Meehl, P. E., and MacCorquodale, K., *Psychol. Rev.*, **58,** 230–33 (1951)
68. Meyer, D. R., *J. Exptl. Psychol.*, **41,** 10–16 (1951)
69. Meyer, D. R., *J. Comp. Physiol. Psychol.*, **44,** 162–67 (1951)
70. Miller, N. E., in *Handbook of Experimental Psychology,* Chap. 13, 435–72 (Stevens, S. S., Ed., John Wiley & Sons, New York, N. Y., 1436 pp., 1951)
71. Montgomery, K. C., *J. Exptl. Psychol.*, **41,** 39–51 (1951)
72. Morgan, C. L., in *Handbook of Experimental Psychology,* Chap. 20, 758–88 (Stevens, S. S., Ed., John Wiley & Sons, Inc., New York, N. Y., 1436 pp., 1951)
73. Morgan, R. L., and Underwood, B. J., *J. Exptl. Psychol.*, **40,** 592–603 (1950)

XUM

74. Mowrer, O. H., *Learning Theory and Personality Dynamics* (The Ronald Press Co., New York, N. Y., 776 pp., 1950)
75. Mowrer, O. H., and Lamoreaux, R. R., *Psychol. Rev.*, **58,** 196–212 (1951)
76. Munn, N. L., *Handbook of Psychological Research on the Rat* (Houghton Mifflin Co., Boston, Mass., 598 pp., 1950)
77. Nissen, H. W., *J. Comp. Physiol. Psychol.*, **44,** 9–16 (1951)
78. Nissen, H. W., in *Handbook of Experimental Psychology*, Chap. 11, 347–86 (Stevens, S. S., Ed., John Wiley & Sons, Inc., New York, N. Y., 1436 pp., 1951)
79. Noble, C. E., *J. Exptl. Psychol.*, **40,** 622–31 (1950)
80. North, A. J., *J. Comp. Physiol. Psychol.*, **43,** 442–60 (1950)
81. North, A. J., *J. Comp. Physiol. Psychol.*, **43,** 461–70 (1950)
82. Postman, L., *Am. J. Psychol.*, **63,** 576–83 (1950)
83. Reed, H. B., *J. Exptl. Psychol.*, **40,** 504–11 (1950)
84. Ribback, A., and Underwood, B. J., *J. Exptl. Psychol.*, **40,** 329–35 (1950)
85. Riopelle, A. J., *J. Exptl. Psychol.*, **40,** 390–95 (1950)
86. Ritchie, B. F., Ebeling, E., and Roth, W., *J. Comp. Physiol. Psychol.*, **43,** 168–80 (1950)
87. Saldanha, E. L., and Bitterman, M. E., *Am. J. Psychol.*, **64,** 37–53 (1951)
88. Saltzman, I., and Koch, S., *J. Exptl. Psychol.*, **38,** 347–70 (1948)
89. Saul, E. V., and Osgood, C. E., *J. Exptl. Psychol.*, **40,** 372–79 (1950)
90. Seward, J. P., *Psychol. Rev.*, **57,** 362–74 (1950)
91. Seward, J. P., *Psychol. Bull.*, **48,** 130–49 (1951)
92. Seward, J. P., Levy, N., and Handlon, J. H., Jr., *J. Comp. Physiol. Psychol.*, **43,** 240–51 (1950)
93. Shaw, M. E., and Waters, R. H., *J. Genetic Psychol.*, **77,** 283–92 (1950)
94. Sheffield, F. D., and Roby, T. B., *J. Comp. Physiol. Psychol.*, **43,** 471–81 (1950)
95. Sheffield, F. D., and Temmer, H. W., *J. Exptl. Psychol.*, **40,** 287–98 (1951)
96. Sheffield, F. D., Wulff, J. J., and Backer, R., *J. Comp. Physiol. Psychol.*, **44,** 3–8 (1951)
97. Sheffield, V. F., *J. Exptl. Psychol.*, **40,** 305–13 (1950)
98. Siegel, P. S., *J. Exptl. Psychol.*, **40,** 604–8 (1950)
99. Simon, C. W., Wickens, D. D., Brown, U., and Pennock, L., *J. Comp. Physiol. Psychol.*, **44,** 67–70 (1951)
100. Skinner, B. F., *Psychol. Rev.*, **57,** 193–216 (1950)
101. Smith, M. P., *J. Comp. Physiol. Psychol.*, **44,** 154–61 (1951)
102. Spence, K. W., in *Comparative Psychology*, Chap. 8, 239–291 (Stone, C. P., Ed., Prentice-Hall, Inc., New York, N. Y., 525 pp., 1951)
103. Spence, K. W., in *Handbook of Experimental Psychology*, Chap. 18, 690–729 (Stevens, S. S., Ed., John Wiley & Sons, New York, N. Y., 1436 pp., 1951)
103a. Spence, K. W., *Psychol. Rev.*, **54,** 1–8 (1947)
104. Spence, K. W., Bergmann, G., and Lippitt, R., *J. Exptl. Psychol.*, **40,** 539–51 (1950)
105. Strange, J. R., *J. Comp. Physiol. Psychol.*, **43,** 194–97 (1950)
106. Strassburger, R. C., *J. Exptl. Psychol.*, **40,** 473–87 (1950)
107. Taylor, J. A., *J. Exptl. Psychol.*, **41,** 81–92 (1951)
108. Taylor, J. G., *Psychol. Rev.*, **57,** 375–89 (1950)
109. Teel, K., and Webb, W. B., *J. Exptl. Psychol.*, **41,** 148–52 (1951)

110. Thistlethwaite, D., *Psychol. Bull.*, **48**, 97–129 (1951)
111. Thune, L. E., *J. Exptl. Psychol.*, **40**, 423–38 (1950)
112. Tsao, J. C., *J. Exptl. Psychol.*, **40**, 639–42 (1950)
113. Tsao, J. C., *J. Exptl. Psychol.*, **40**, 752–55 (1950)
114. Underwood, B. J., *Am. J. Psychol.*, **43**, 594–99 (1950)
115. Underwood, B. J., and Hughes, R. H., *Am. J. Psychol.*, **63**, 422–30 (1950)
116. Vinacke, W. E., *Psychol. Bull.*, **48**, 1–31 (1951)
117. Voeks, V. W., *J. Psychol.*, **30**, 341–62 (1950)
118. Weise, P., and Bitterman, M. E., *Psychol. Rev.*, **58**, 185–95 (1951)
119. Woodbury, C. B., *J. Comp. Physiol. Psychol.*, **43**, 490–502 (1950)
120. Woodbury, C. B., *Psychol. Rev.*, **58**, 18–29 (1951)
121. Zeaman, D., and House, B. J., *J. Exptl. Psychol.*, **40**, 570–83 (1950)

XUM

VISION[1]

By Harry Helson

Department of Psychology, The University of Texas, Austin, Texas

Since the first two annual reviews of vision [Bartlett (6); Chapanis (17)], many new publications dealing with its various aspects have appeared. They deal with such disparate subjects as quantal theories of visual processes (having as the object of their consideration the ultimate packets of energy and their role in vision), the lack of correspondence between physical and perceived space, and the fundamental abilities comprising visual imagination! It would be much easier to write a review concerned only with the highlights in the field, but this is manifestly impossible, if not unfair. A reviewer must attempt to give an over-all picture while including as much detail as possible. He must keep in mind not only the specialist but, in a volume such as this, the nonspecialist also who is interested in finding out what has occurred of importance to him in the field. The general reader should find something of use even among technical details. To give an example, it is usually believed that no prediction can be made regarding the color of pigment mixtures from the colors of the components. Duncan's paper (24) discussed below has changed all that. If the account here does not completely clarify all the topics under discussion, it is hoped that it may at least provide leads for further reading.

General

Books and summaries.—Not many books have appeared during the past year in the field of vision. To the writer's knowledge, only two or three were published by psychologists.

At last we have a book on the embryology of the human eye. Mann (74) supplies a much-needed account of the development of the human eye with some reference to phylogenetic development. She discusses the origins of the lens, vitreous, suspensory ligament, and the orbit and its contents. There is discussion of congenital anomalies and a synoptic comparison of ocular with general development. Ogle (87) now makes available in a book couched in less technical language and without much of the difficult mathematics, his researches in binocular vision and space perception which many have known in his journal articles. Space does not allow even a short summary of the contents of this book which every vision psychologist will want to study. Two points deserve special mention: Ogle asserts that stereopsis may be present for distances up to 650 m., a value which is far beyond that commonly assumed possible. In line with his emphasis on and researches into aniseikonia, Ogle points out that only objects in the median plane

[1] The survey of the literature pertaining to this review was concluded in April, 1951.

can give rise to equal-sized images on the retinas, and when the eyes are turned to see objects to right or left, the image is larger in one eye than in the other because of the difference in distance from object to the two retinas. To the psychologist, this raises the question whether this difference is great enough to be above threshold, and if it is above, whether it is not counter-balanced by other factors. A more general book is Adler's book on the physiology of the eye (1). It is written sufficiently simply and at a level where it could be used in courses in physiological psychology or vision.

The book by Knox & Ellerbrock (63), while primarily intended for optometrists, can be profitably used by psychologists since it deals with light stimuli and image formation, the visual mechanism, correction of refractive defects by means of lenses, and optometric procedures for refraction and motor anomalies. The books by Mann, Ogle, Adler, and Knox & Ellerbrock furnish a solid background for work in present-day physiological optics.

The book by Pickford (91) on individual differences in color vision is a real contribution, since it stresses many psychological aspects which have not been emphasized by others. Pickford gives detailed study to individual differences in color vision and arrives at the conclusion that "about 6–8% whites are extremely different from the majority and actually form six sub-groups, all of which are extremely different from each other" (91, p. 305). He explodes the idea of a general color ability, since ability to distinguish one set of colors, say reds, does not imply equal ability to distinguish yellows, greens, or blues, a finding which will be bad news to industry which now looks for individuals with "superior" color discrimination in all hues. The role of inheritance is stressed in this book, and the Hering theory with some modification is regarded as a theory which fits the facts with astonishing ease and which requires no special pleading to support its claims. Certainly this is a book by a psychologist for psychologists!

The phenomenological approach to space perception is to be found in pure and pronounced form in Gibson (28), who deals almost exclusively with distance and space and hardly at all with color. His thesis is that gradients of texture are the primary determinants of visual space. The book is rich in illustrations, but the lack of quantitative data will make it difficult for many readers to follow the argument and to appreciate fully the contributions which this book undoubtedly makes to visual science.

Two excellent presentations of colorimetry are to be found in Judd (59, 60) with many applications of the International Commission on Illumination (ICI) system to color measurement, color specification, and color mixture and to systems based on the primaries of the Young-Helmholtz, Ladd-Franklin, and Hering theories of color vision. His discussions of indices of hue, lightness, and saturation, of whiteness measurements, and of material standards of color show how the colorimetrist approaches problems of color with quantitative and instrumental methods.

Reviews of electro-oculography and peripheral visual acuity [Low (70); Marg (75)] should also be mentioned. Marg gives considerable information

regarding corneoretinal potentials, electrodes and instruments employed in their detection, and the recording of eye movements by electro-oculography. Similarly, Low's review is very informative concerning methods of measuring acuity with particular reference to peripheral visual acuity.[2]

Apparatus and demonstrations.—The field of vision offers many opportunities for new demonstrations, some having theoretical interest, others a means for study of other phenomena, and still others mere curiosities at the moment. One of the most striking demonstrations which has long been known to physicists, and perhaps to some psychologists, is a very simple device consisting of an episcotister which rotates in front of a red and a green lamp. The disk is half black, half white, except for a 30° opening allowing the lamps to be seen as the disk rotates. When rotated in one direction [as described by Lehmann (68)], the lamps are seen in their "true" red and green color, but when rotated in the opposite direction, the red lamp is green and the green lamp is red. Obviously this is an "after-image" phenomenon, but why does it arise with change in direction of rotation? The adaptation-level theory offers the simplest explanation of this phenomenon, provided one does not ask what actual photochemical or other processes are involved. Lehmann points out some interesting dependencies of the phenomenon on various conditions.

Not unrelated to this demonstration are a number of phenomena described by Asher (2) with stereoscopic devices and rotating disks. He believes retinal mechanisms, chiefly inhibitory in character, are responsible for the effects. Owing to the lack of quantitative data, it is difficult to regard the effects he describes as more than interesting demonstrations at the present time. Also in this vein is a clever demonstration of flicker beats analogous to tonal beats described by Attneave & McReynolds (4). They use two

[2] After this review was completed, the *Handbook of Experimental Psychology* edited by Stevens (108) reached the writer. Consequently, the specific contributions (and there are many) in the chapters dealing with vision cannot even be indicated here. The chapter by Judd, "Basic correlates of the Visual Stimulus" (108, pp. 811–67), contains a discussion of radiometric, photometric, and colorimetric terms and procedures, with particular reference to the ICI system, the Munsell system, and discussion of various problems in color vision, particularly constancy, color conversion, and related topics. The chapter by Graham, "Visual Perception" (108, pp. 868–920), deals with cues to depth perception, visual angle, and size constancy, accommodation and convergence, the horopter, stereoscopic vision, perception of movement, span of perception, and figural after-effects. Some very original treatments of problems of space perception will be found in this chapter. Bartley's chapter, "The Psychophysiology of Vision" (108, pp. 921–84) deals with optical mechanisms, anatomical mechanisms, photochemical processes, neural mechanisms, optic nerve and cortical action potentials, and oculomotor mechanisms. Certainly these chapters along with much material relevant to vision in other chapters make this a must book for every scientist working in the field of vision. The book as a whole is one of the most notable contributions to psychology and, in a sense, shows how the stage is set for the experimental psychology of the second half of this century.

XUM

strobotacs to cast flickering lights on a surface. By adjusting the rates of flicker, visual beats are readily obtained. A black-white disk which yields "subjective" colors is described by Hess (43) and can be used with large groups to give predictable results. A five-pointed star is described by Scheidemann (100) to be such that all sides really change direction when viewed in a mirror. In the traditional six-pointed star, one-third of the lines do not furnish any test of mirror-tracing because no change in spatial direction is involved. Interesting entoptic phenomena due to erythrocytes in the retinal circulation are described and explained by Chatham (18).

Dimensional Processes

Stimulus correlates.—Until very recently, it has been necessary to restrict the laws governing color mixture to mixtures of lights because the color of a mixture of pigments could not be predicted from any information regarding the components of the mixture. A paper by Duncan (24) shows that a good beginning has been made toward predicting pigment mixtures from a knowledge of the relative values of the coefficients of absorption and of scatter or diffusion of the components at a number of wavelengths throughout the visible spectrum. Formulae have been developed based on these coefficients to give the predicted reflectance/wavelength curve from which the predicted color according to the ICI system can be readily calculated. The theory not only predicts the colors obtainable with varying percentage composition of a given set of pigments but also what mixtures are necessary to match a given color, what factors determine brightness, saturation, and other properties of mixtures. It is of interest to note that whereas only three properly chosen wavelengths are necessary to match all colors (as aperture colors) only four pigments are necessary to match any surface color. Lest anyone gain the impression that formulae are available for perfect prediction of surface colors, it should be pointed out that Duncan is fully aware of problems arising from differences in various properties of surfaces which his theory does not yet completely predict.

Usually, scientific work in vision entails the use of standard light sources, and the most convenient method of specifying these is in terms of equivalence with black body radiators at determinate color temperatures. Thus, the standard illuminants, A, B, and C, recommended by the ICI are equivalent to black body radiators at specified color temperatures. Owing to a change in the radiation constant necessitated by the use of the Planck radiation formula in place of Wien's, previously in use, the scale of color temperature adopted in 1949 yields new values of color temperature for standard illuminants, as Judd has pointed out (62). Thus, standard illuminant A, which was formerly rated as having color temperature of 2,842° K., is now rated according to the new formula as having a color temperature of 2,854° K. Judd gives formulae in this paper for converting color temperature values according to earlier conventions into the present scale. Also this paper gives formulae for adjusting the voltage on lamps rated according to earlier scales so that they will conform to the present specifications.

XUM

One of the most important ways of specifying color stimuli is by means of the ICI system based on the properties of the so-called standard observer. Some years ago, Jacobsen found x, y chromaticity coordinates of two paints were almost identical although their spectrophotometric curves were markedly different in the spectral region below 430 mμ and the paints were also markedly different in perception. This meant that the standard observer did not weight the extreme short-wave portion of the spectrum enough. However, in repeating measurements on two titanium dioxide pigments, Judd (57) found that four of his observers corroborated Jacobsen's findings and three others checked the standard observer! The task of modifying the standard observer would now seem to be more difficult than ever in the light of these findings since they suggest that no average luminosity function can represent all normal observers. Thomson (111) who also has discussed the shape irregularities in the equal energy luminosity curve confirmed the original Gibson & Tyndall findings that the luminosity function is higher in the extreme short-wave portion of the spectrum than the ICI function assumes it to be, and he asks, "Since the standard ICI curve was based on the Gibson & Tyndall data, why was it smoothed in the blue and violet region?"

Finally, of importance for standardization of work in vision are the recommendations concerning the experimental conditions for investigations on visual sensitivity and discrimination [Ballard (5)]. The conditions set forth are not meant to be the only conditions for work in physiological optics, and it is hoped that they will be discussed and perhaps changed. The recommendations concern shape and size of test object and contrast of test object and background, spectral composition of light on both test object and background, background brightness, angle of field of view, normal observers with standard adaptation or pre-adaptation conditions which are also defined and the treatment of thresholds as probability values. Nothing is said, however, regarding the training or instructions given the observers, the choice of proper psychophysical methods for the problems investigated, or the importance of attitude and criterion of report. The fact that standardization has reached this far is a cause for congratulation and should lead to more uniform experimental conditions and to fewer uncontrolled variables in experiments dealing with visual functions of all sorts.

Dimensions of vision.—The old problem of the number of attributes of sensation persists into the present under a number of different guises. Since the classic work of Katz in 1911 on the modes of appearance of colors (*Erscheinungsweisen der Farben*) it has been generally accepted by psychologists that the three dimensions of hue, brightness, and saturation are sufficient for describing only colors seen in small apertures. But how many more attributes or dimensions should be recognized depends upon the definition both of color and of dimension. Helson (38) has argued for as many as 18 dimensions of color. He lists pronouncedness, impressiveness, size, location, volume, hardness, warmth, gloss, transparency, texture, luster, sparkle, flicker, shape, and affective tone among the dimensions of color along with

the traditional attributes belonging only to aperture colors. Most colorimetrists, following the lead of the Colorimetry Committee of the Optical Society of America, prefer to regard all but the traditional attributes as "modes of appearance" or perceptual modes, thus drawing a sharp line between psychophysical data and perceptual data. Of course most psychologists know that this cannot be done. Many of the "modes" can be treated by psychophysical methods as well as brightness, hue, or saturation. Gibson (27) lists eight fundamental dimensions of vision, but reduces them all to gradients of texture in the retinal image. This seems like an oversimplification of a complex problem.

Reverting to earlier views, Evans (25) maintains that white and black are not colors and do not constitute a series. White in his view is a function of the diffusion of light which at one extreme is represented by clear (nondiffusing) and proceeds through pale to maximum white (think of milk being added to water). White may vary in brightness and strength. Gray requires at least two areas and is a perception of "visual effectiveness." Black represents the extreme lack of effectiveness. Hence, white, gray, and black are not sensory modes but object modes. Psychologists, in general, will not be satisfied with this analysis, even though it does emphasize certain new facts. It does not account for the achromatic quality of textured surfaces, it fails to recognize that gray may be seen when only one surface is present in the field of vision, that "sensory modes" are also affected by the presence of other areas through simultaneous contrast or color induction, and that black is a positive color and can be as much influenced by surroundings as any other color.

An extensive series of experimental investigations of white has been reported from the Eastman Kodak laboratories by Hurvich & Jameson (49). These writers tested the luminances at which a series of sources varying from 2,848° K. to 10,000° K. were reported white at luminances ranging from 3 to 3,000 mL. (millilamberts) for an eye neutrally adapted or adapted to illuminants of different color temperatures. The systematic variation of conditions, too numerous to be detailed here, and the range of the variables investigated make this a model of scientific procedure in the field of visual science. This study verifies again the chief conclusion of Helson & Michels (40) that threshold luminance for achromaticity is usually minimal at that color temperature of the test field which is either identical to or approaches the color of the adapting illuminant. Whereas Helson & Michels found this to be true for strongly chromatic surrounds, Hurvich & Jameson (49) found it for fields which were at best weakly chromatic (2,848 to 10,000° K.). This study shows that thresholds for white are affected by size, color temperature, and time of exposure of test object and surround. White is thus seen to behave essentially like chromatic colors.

The problem of predicting the total similarity or difference of objects from a knowledge of their separate dimensions is tackled by Attneave (3). His work represents an important start toward what may be called multi-

XUM

dimensional psychophysics. Using (*a*) parallelograms varying in size, in angularity, and sometimes in color, (*b*) gray squares varying in area and reflectance, and (*c*) triangles varying in area and triangularity, Attneave concludes that where the range of variation is not too great, the judgments are amenable to metric treatment but not by Euclidean spatial representation or simple additive hypotheses. Further work along this line might also clarify many problems in unidimensional psychophysics.

Dependence of brightness on shape and area.—Hanes (33) found that if two stimuli of the same area but of different shape are equated for brightness, triangle and circle appear brighter than square at certain levels. The effect of shape on brightness is neither simple nor univocal for with the largest size studied the circle appeared brightest. The results, as Hanes points out, are affected also by differences in perimeters of the figures, their maximum dimensions, and their apparent sizes. In a later study, Hanes (34) investigated suprathreshold relations of area and brightness. The subjects adjusted a variable luminous disk to equal a standard in brightness. Five sizes were used as both standards and variables: stimuli subtending angles of 9, 18, 36, 72, and 144′. The luminances of the standards were 0.1, 1.0, 10.0, and 100.0 mL. At lower levels (from 0.1 to 1.0 mL.) brightness increases with size, at 10 mL. the brightness drops as size is increased from 9′ to 50′ and then rises slightly with an increase to the maximum size of 144′. Hence, to have relatively small luminous stimuli of different sizes appear equally bright in a dark field, their luminance must be altered in a manner depending on size and luminance level.

Psychophysical Methods and Judgments

The significance of the psychophysical method used in determining threshold data is brought out in a study by Sperling & Farnsworth (106). The difference limen (DL) from a single point in the ICI chromaticity plane was determined by four different methods: (*a*) method of constants, (*b*) method of constants with direction of difference indicated, (*c*) method of paired comparisons, and (*d*) method of adjustments. All methods gave significantly different DL's at the 1 per cent confidence level. The smallest thresholds were obtained with the method of adjustments and the constant method with direction. The DL for a difference in chromaticity was unaffected by the surround brightness within the range studied. A psychophysical study by Newhall (86) was made to obtain uniform, psychological spacing in lightness. This study, among others, shows how interest in constructing psychological scales has spread from the field of hearing to that of vision. So far, however, most scale construction has been purely empirical, devoid of any theory of the basis by which equal psychological steps are obtained. Such a theory has been proposed by Michels & Helson (76) and has been tested by Nash (85). The theory relates the judgmental category scale to the stimuli being judged by assuming that the slope of the linear function between judgments and log stimulus intensity depends upon the

number of categories in the judgment scale. When the number of categories increases above a certain limit, the slope of the line should approach a constant independent of the number of categories. This limit is fixed by a value called the judgment constant which depends upon the experimental conditions of judging. The adaptation level, on the other hand, is assumed to be independent of the number of categories employed in its determination. Nash verified these assumptions using weights as stimuli. They should also be tested with auditory and visual stimuli.

Saltzman & Garner (99) found that angles below 45° were overestimated while those above were underestimated, a finding in keeping with the general principle that stimuli below adaptation level behave differently from those above. Their further finding that an additional reference angle affected the variable but not the constant errors suggests some interesting speculations when viewed from the vantage point of a general theory of such phenomena.

Basic Processes and Mechanisms

Dark adaptation.—Commonly accepted notions about the increased sensitivity of the eye after dark adaptation are shown to be questionable if not wrong by Rose (96) who points out that lowering thresholds by a thousand to ten thousandfold does not mean that the eye is a thousand to ten thousand times more sensitive at low illuminations than at high. Computations of the concentrations of photosensitive material in the retina on the basis of such measurements may lead to wrong conclusions because only a small part of the range of dark adaptation can be ascribed to the true sensitivity of the eye. He points out that the chief mechanism for dark adaptation lies not in the variable absorption of light by photosensitive material but rather in intermediate and secondary processes whereby the energy of the absorbed quanta is amplified to a much higher level of nerve pulse energies. Research might therefore be more profitably directed toward these secondary, amplifying processes than toward the primary process of light absorption. The problem is one of the gain of the visual system, and this must not be confused with its sensitivity. Quantal considerations and comparisons of the eye with television pick-ups yielding comparable sensitivities confirm this interpretation, according to Rose.

Another study which upsets some commonly held notions regarding dark adaptation is that by Mitchell, Morris & Dimmick (79) who measured dark thresholds after varying periods of prior red adaptation. The course of dark adaptation was traced after different periods of red adaptation under the following conditions: The subject adapted first to 150 mL. wearing red goggles having a transmittance of 10.5 per cent for the 2,848° K. source, giving an effective level of 16 mL. He was first in the 150 mL. for 10 min., then put on goggles for either 0, 2, 3.5, 5, 10, 20, or 40 min. after which dark thresholds were measured. Table I shows the initial sensitivity after seven different periods of red adaptation.

XUM

TABLE I

EFFECT OF RED ADAPTATION ON BRIGHTNESS SENSITIVITY*

Time in red goggles (min.)	Sensitivity level after 5 sec. in dark log $\mu\mu$L.
0	7.48
2	7.04
3.5	7.04
5	6.87
10	6.59
20	6.38
40	6.26

* From Mitchell, *et al.* (79).

Red adaptation does not give as great an increase in sensitivity as does adaptation in darkness for an equal time. The sensitivity level reached after 20 min. red adaptation plus five sec. in darkness is reached in 35 sec. in darkness alone! It would therefore appear that the time saved in dark adaptation following the use of red is not very great.

A study by Mote, Riopelle & Meyer (80) and one by Mote & Riopelle (81) are concerned with the effect of intermittent pre-adapting light upon subsequent dark adaptation. Usually the pre-adapting stimulus is continuous, but here the object was to study the effect of discontinuous stimulation on subsequent dark adaptation. In the first study, nine brightnesses ranging from 8,860 to 22.15 mL. were used at four flicker rates ranging from 2 c.p.s. to $\frac{1}{4}$ c.p.s. Under these conditions, the flashes were either so bright or the intervals were so short there was little or no recovery. The results approximated results for continuous light adaptation. In the second study, an additional variable was introduced, the light-dark ratio, which had been kept constant before at $\frac{1}{2}$ to $\frac{1}{2}$. Thresholds were determined following preadaptations to light-dark ratios of $\frac{1}{2}$ to $\frac{1}{3}$, $\frac{1}{4}$ to $\frac{1}{8}$, $\frac{1}{8}$ to $\frac{1}{16}$, and $\frac{1}{16}$ to $\frac{1}{32}$. The difference between experimental and control data can be expressed as follows: as the amount of light time in the ratio decreases, the value of the threshold decreases. The threshold is about 1 log unit lower initially for $\frac{1}{16}$ than for $\frac{1}{2}$ light time. The total amount of light reaching the eye was the same for all light ratios due to the fact that the periods of stimulation were inversely proportional to the light times. However, the dark adaptation curves approach a common level for all light-times after about 20 min. of dark adaptation for a given level of pre-exposure. The upshot of this study would seem to be that we cannot regard the time integral of the light as a valid index of the intermittent stimulation under the conditions of this experiment. However, this should not mean that the time integral of the

intensity never affects the threshold, for under other conditions it may do so (38a).

A paper by Sloan (105) turns attention from stimulus conditions to the retina. She shows that the rod threshold of the dark-adapted eye varies inversely with the areal density of the rods. This is not the case with cone thresholds where the threshold is not closely related to cone density in either light or dark adaptation.

The effect of vitamins on dark adaptation was studied by Millard (78) who finds that vitamin A_2 lowers the red threshold in the dark-adapted eye about 30 per cent. No change was found in the threshold for blue. A control group deprived of this vitamin showed a rise of about 19 per cent for both red and blue stimulation. This study stands almost alone in its demonstration of an effect of vitamin intake on color sensitivity. Feldman (26), on the contrary, reports that the belief of some myopes that vitamins and/or minerals improved their vision was not borne out by actual tests. The fact that drugs may have injurious effects on vision has long been known and receives new clinical significance in the report of Thomas (110) that the most frequent change in visual fields in cases treated with streptomycin was a nerve bundle type of scotoma. It would be interesting to know what percentage of individuals treated with streptomycin and other antibiotics develop visual abnormalities and how long they last. Certainly the literature gives no clear picture at present of clear-cut generalizations regarding either the beneficial or harmful effects of the biologically active substances so widely used in present-day medical practice.

Before closing this section, reference must be made to two studies which do not relate particularly to the others. Hartridge & Weale (37) duplicated the scintillation of stars in the laboratory by means of a small light source and found that neither very bright nor very dim sources scintillate. They assume that this finding is due to eye movements, apparently unaware of the numerous studies by psychologists showing that eye movements alone do not account for similar phenomena commonly called "fluctuations of attention." The phenomenon is a general one found with stimuli near the threshold in all sense modalities. Guilford (31) showed that such fluctuations are not due to eye movements and that they conform to the phi-gamma hypothesis. Stimuli above the threshold are visible more than half the time, stimuli below the threshold for less than half the time, while stimuli at threshold fluctuate with greatest frequency. The phenomenon can be easily demonstrated by means of a Masson disk. Of course, the scintillation of stars viewed in open air may be complicated by warm and cold air currents which cause movement in the perceived image, but apart from this, the phenomenon is probably largely an artifact of the threshold. As the stimulus goes below threshold, an eye movement can bring it back, but the threshold is the determining factor.

A phenomenon of night vision that has occasioned a good deal of speculation is the near sightedness developed by the eye in dim vision. Better night

XUM

vision is obtained when negative spectacles are worn. The amount of myopia found in night vision has been variously reported from 0.59 to 1 or 2 diopters, probably because of differences in light levels, methods of measuring the myopia, etc. After examining critically the chief theories and explanations of the phenomenon, Koomen, Scolnik & Tousey (65) conclude that it is due to undercorrected spherical aberration of the eye. They base their conclusions chiefly on the fact that night myopia persisted after prevention of accommodation by optical methods and by use of homatropine (evidence by exclusion of accommodation) and could be produced by means of a lens system having the spherical aberration properties of the eye (evidence by production of the postulated cause). It is a little hard to accept this conclusion after their own criticism of the spherical aberration theory made earlier in the same paper. The problem is not closed by this study.

Quantum considerations in vision.—Studies of dark adaptation have been a fertile area for the application of quantum theories for obvious reasons. Area, intensity, and time of stimulation have been extensively investigated in dark adaptation, and with the small, often minute, amounts of energy involved at the absolute, dark-adapted threshold, it is natural to envisage the experimental findings within a quantal frame of reference. Difficult as quantum theory may be mathematically, it offers the neatest and simplest conceptual framework for stimulus-threshold relationships since only light flux per unit of area and time enter into consideration as independence of quanta and of retinal units is assumed. In brief, quantum theory furnishes an approach to visual problems based essentially on probability mathematics and hence ties up with the historical approach to psychophysical problems. Great impetus was given to the quantal approach by the late Selig Hecht and his co-workers [cf. especially Hecht, Shlaer & Pirenne (37a)] who claimed that the percentage of positive reports as a function of the log stimulus intensity can be represented by a cumulative Poisson curve. During the past year, several papers have appeared attacking this position both on experimental and theoretical grounds. Crozier (20), in a lengthy experimental-theoretical paper, presents results in which the threshold (considered as a mean value) and the variance vary independently, a finding contrary to the Poisson assumptions. On the other hand, Crozier's data do seem to fit a log Gaussian integral and hence might still support a quantum interpretation since quantum theory is not limited to a single probability distribution (e.g., the Poisson). But Crozier believes that vision is too complicated to be accounted for in terms of Hecht's theory.

While criticizing formulations of intensity discrimination using quantum concepts employing Poisson distributions, Mueller in two papers (83, 84) takes a somewhat intermediate position. He agrees that quantum variability found in visual experiments may furnish a baseline upon which all visual mechanisms must be considered to operate but that it is not possible to explain all the data of vision on quantum concepts alone. He points out that a normal distribution approximates the Poisson even when the mean number

of quanta is as low as 10 or 15. He then proceeds to develop a quantal theory in terms of the critical differences in stimulus quanta necessary for the perception of just noticeably brighter stimuli rather than in terms of sums as others have done. In his second paper (84), Mueller presents data based on perception of brighter for an increase in intensity of a field to which the eye is already adapted foveally and concludes that either the Hecht Poisson or the Crozier log Gaussian integral fits the data in a satisfying manner. But other consequences than the shape of the psychophysical function must decide which theory is accepted. Because quantum theory does not account for adaptation effects found operative in intensity discrimination, Mueller asserts that the absorption constant and other factors must be brought in to supplement quantum theories of vision. Papers such as those by Bouman (11) and Baumgardt (7) should be read in the light of the criticisms raised in the papers of Crozier and Mueller against quantum theory. On the other hand, it is probably safe to accept two quanta as the minimal number that must be absorbed by the rods to elicit a visual response [Bouman & van der Velden (12)].

Though not directly related to quantum theory, the study of Casperson & Schlosberg (15) may properly be brought in at this point because of their attempt to apply a probability theory to the perception of one to seven dots under monocular and binocular conditions of observation. A probability theory fits the monocular data well, but the assumption of compounded monocular probabilities for binocular vision was not verified. This paper may be cited in support of Mueller's and Crozier's position that probability theory alone will not suffice for all the facts of visual discrimination.

Chemistry of vision.—In a series of papers, Wald and his co-workers (113, 114, 115) have reported studies of absorption of light by rhodopsin, the synthesis of rhodopsin from vitamin A, the cycle of rhodopsin to retinene plus protein to vitamin A_1 plus protein to rhodopsin, and the role of the pigment epithelium in regeneration of rhodopsin. The latter is pregnant with new possibilities for insight into the mechanism of vision because the importance of the pigment epithelium has not generally been recognized. Wald (113) reports that isolated frog retinas and retinal homogenates regenerate rhodopsin in amounts about 10 per cent as large as are formed during dark adaptation *in vivo*. But on addition of pigment layer homogenates, the retinal homogenates synthesize rhodopsin in high yield. The only vitamin A_1 available to them for this process is supplied by the pigment epithelium. This structure, therefore, would seem to play an integral part in visual sensitivity as Helson & Guilford (39) long ago asserted on the basis that dark-adaptation thresholds were lower in dark-eyed human subjects than in light-eyed subjects. Limiting the role of the pigment layer to absorption of light with its only purpose being that of reducing scattering would seem to be an undue restriction of its function. On the other hand, it must be remembered that the retinal pigment may not serve identical functions in the frog and human retinas. Only further research can settle the question as to its precise role in brightness and color vision of humans.

XUM

Critical fusion frequency.—Data concerning the ability of the eye to receive and transmit impulses are now of practical as well as theoretical importance in view of the emergence of television as a medium of entertainment and instruction. A report by a Senate Advisory Committee on Color Television [Condon (19)] compares three rival systems of color television. It explains the mechanics and gives the bases for acceptable color television. Consideration of how many dots a satisfactory television picture must contain, how many pictures per second must be transmitted, the role of primary colors in fidelity of reproduction, and the methods of assessing the three rival systems could not fail to bring in comparisons of natural vision with television and the relations of the two. It is pointed out that whereas much has been done on critical fusion frequency, little or nothing has been done regarding tolerance for flicker. Hence, the people called in by this committee to act as scientific consultants had to construct their own tolerance scale. Probably unaware of recent developments in psychophysics, these consultants hit upon a rating-scale type of report for estimates of effects of flicker. The scale was: none, noticeable, appreciable, objectionable, and painful flicker. They found that none-to-noticeable flicker moves up in rate with the brightness level (as was to be expected) but so also did the painful and objectionable rates! The shifts affected the whole scale as the adaptation level changed. If television is to be really satisfactory, it must be able to deliver better than 4.3×10^6 bits of information per second, the maximum value calculated by Jacobson (53) for the informational capacity of the human eye.

In recent years, critical fusion frequency (cff) has assumed an importance far beyond the insights it has given into visual functions. Tanner (109) concludes from experiments which show correlation between cff and American Council on Education test scores that the shortest noticeable dark period for a light flash of some critical length promises to be a significant correlate of intelligence. Until some basic reason is advanced for such correlations, it is doubtful if they will be accepted by hard-headed physiological psychologists as more than suggestive leads toward further research. Take for example the finding by Miles (77) that advanced age is not necessarily accompanied by depressed cff. Would it be safe to say that there are or are not corresponding changes in intelligence with changes or lack of change in cff? Miles' subjects might furnish a good test for Tanner's hypothesis.

Color discrimination and color blindness.—Since Abney reported in 1910 that hue does not remain constant when highly chromatic light is mixed with white light, it has been known that the loci of the constant hues in the color mixture diagram are curvilinear, not linear, except for yellow and violet perhaps. The loci of constant hue vary with the state of adaptation of the eye as MacAdam (73) shows in his determination of constant hue loci with various surrounding colors (or, various states of chromatic adaptation). Using various chromatic surrounds, MacAdam diluted chromatic light with white light, varied both to keep hue and brightness constant, and found curved loci running from the original, saturated point in the mixture diagram

to a white point dependent on the state of adaptation. The convergence points for different adapting hues were, as Helson & Michels (40) had previously shown, toward the point representing the color of the surround. MacAdam also found that the law of additivity of luminosities does not apply to brightness of mixtures (73, p. 591):

> When a white mixture was kept unchanged in the comparison half of the field, and the red component of an equally bright white mixture was reduced in intensity, the resulting color was a bluish-green, very obviously brighter than the white.[3]

The average point of convergence of all the constant hue loci was also the point chosen white in the dark room when the comparison field was extinguished, a finding also coinciding with the Helson & Michels study. The now well-known series of ellipses encompassing equal numbers of just noticeable differences for the various hues and chromas found previously with dark surround were shown in an earlier study by MacAdam (72) to vary with color of the surrounding field. Hunt (48) has found that adaptation to illuminants A or B yields more saturated hues than are observed with dark-adapted eye. The chromaticity co-ordinates of some of the colors under light adaptation are outside the spectral locus which supposedly is the boundary of all "real" colors! Monochromatic stimuli, hence, do not yield maximal saturations under all conditions, but only for small, foveal areas when the rest of the eye is unstimulated. We need a standard observer and a coordinate system for all field conditions comparable to the ICI system for aperture colors. Working toward this goal, Helson, Judd & Warren (41) give equations predicting the colors of objects in Illuminant A from their chromaticity co-ordinates in Illuminant C by taking into account the altered adaptation of the eye in Illuminant A.

The demand for consideration of field conditions in colorimetry has come not only from psychologists interested in a general approach to color problems but also from workers in industry who are interested in "common viewing conditions" as opposed to the conditions usually employed in colorimetric and other instrumental studies. Thus, Davidson (22) points out that determinations of thresholds for differences in brightness, colorimetric purity, and hue, have, in general, yielded values too small to be applicable to everyday viewing conditions. Instead of using self-luminous colors as in colorimeters, spinning disks, and optimal viewing conditions, Davidson resorted to colored woolens carefully prepared and judged by highly skilled subjects on a rating scale. While the lightness limens so obtained were larger than those reported by previous workers with the usual conditions, the chromaticity limens were about the same as those reported from instrumental studies. Unfortunately, differences obtainable through the use of a rating scale method are largely determined by the number of categories allowed the judges [Michels & Helson (76)], and so Davidson's results must

[3] For the importance of the additivity of luminosities for color theory, see Sinden (103).

XUM

be regarded as chiefly an artifact of his method. Furthermore, better approximations to everyday conditions are not made by duplicating the uncertainties in real life but by better designed experiments.

The topic of color blindness forms a natural part of the more general subject of color discrimination since it represents an altered state of the color discriminating mechanism. The application of modern colorimetric methods to the data yielded by color weak, color blind, and color deviant individuals has heralded a new era in the study of abnormal color responses. Thus, Judd (56) has been able to show not only what colors are confused by protanopes and deuteranopes, respectively, but also has been able to state in quantitative terms what colors are and are not perceived by these types and to compare them with the normal observer through an ingenious transformation of ICI color mixture data to Munsell notations. An error commonly made concerning the vision of protanopes and deuteranopes, namely, that protanopes, blind to red, nevertheless see green and deuteranopes, blind to green, nevertheless see red, is refuted both by Judd in the study just referred to and by Pickford (91). As Pickford and Judd flatly declare, both types of individuals simply do not see either red or green, the differences between them depending upon the type of luminosity function they possess. The error has perhaps arisen from the failure to distinguish between the color weak (protanomalous and deuteranomalous) and the color blind (protanope and deuteranope).

From the results of an item analysis of the Ishihara test, Pickford (90) concludes that while it serves the major purpose for which it was designed, namely to detect major red-green defectives, it cannot distinguish among the sub-types or diagnose the small differences among color deficient individuals. Indeed, according to Pickford, no isochromatic color tests, wools, beads, or lanterns will make the finer distinctions among color deviants. For this purpose, resort must be made to the anomaloscope. In his book, already referred to, Pickford (91) shows that color discrimination whether normal or abnormal is not a single trait or capacity of the organism. It is as complicated and unique as other traits and capacities composed of several factors.

That color blindness may be caused by factors other than the color mechanisms supposedly located in the retina, optic nerve, or brain is shown in a study of congenital tritanopia by Judd, Plaza & Farnsworth (61). This form of color blindness is usually found in diseases of the retina and is characterized by confusions of gray, greenish-yellow, and purplish-blue. The case described by these writers is shown to be due to abnormally heavy pigmentation (five to six times normal) of the lens and seems to have been present from birth.

Color theory.—Discussion of color theories centers around the various forms of the Young-Helmholtz trichromatic formulation, chiefly because of its conceptual simplicity, since it employs the fewest number of basic or primary excitation processes, and because of its incorporation in the ICI and similar mixture diagrams. While it is possible to express color mixture

data in terms of four or more color primaries, as Judd has shown (60), the necessary computations must at present first be made in terms of the three-component theory and then transformed by means of messy equations into systems based on more than three primaries. Until other theories can be expressed in as simple quantitative form as the Young-Helmholtz theory, they are not likely to win the general acceptance accorded this theory in spite of all its known shortcomings in explaining the facts of vision. Some authorities would hold, however, that this theory is still the best, apart from its superiority as a quantitative instrument. Wright (120), after surveying the main facts of vision, concludes that some form of trichromatic theory can account for most of the phenomena of color in so far as receptor processes may be responsible for them. But other, equally competent workers deny this on various theoretical and experimental grounds. Hunt (47, 48) found that when the left eye was adapted to illuminants A or B, it gave more saturated colors than the right eye kept in dark adaptation. He writes that

> no amount of juggling with the overall sensitivities of the three receptor mechanisms could possibly result in arrows going simultaneously in all directions outwards from the center

of the color mixture diagram. Alternatives to the trichromatic theory are Hartridge's polychromatic theory (36, 37) which employs some seven primaries and the more involved theory of G. E. Müller which is now being espoused by Judd (58). In spite of the fact that Judd has worked out coordinate systems for normal and color defective vision in terms of Müller's three-stage theory (58), it is still about as complicated as the facts to be explained.

Unexpected support for a trichromatic theory comes from a study of cortical action potentials arising from electrical stimulation of the optic nerve in cats by Chang & Kaada (16). Such stimulation results in six successive deflections (picked up from the gyrus lateralis of the cortex) varying in amount, latency, form (mono-, di-, or triphasic), and sign (plus or minus). These deflections are differently affected by pressure, strychnine, anoxia, etc. Analysis of these potentials coming over a triple pathway between the lateral geniculate body and the cortex invites speculation concerning their relation to color vision since they are found mainly in higher mammals which are able to recognize colors. The authors suggest that the triple optic pathways might each mediate chiefly the impulses of one of the fundamental components of trichromatic vision. Such studies as this should set an upper limit on the number and complexity of fundamental visual mechanisms which may be postulated to explain the facts of color vision. Jones & Jones (55) also lend support to a three-component theory by a factor analysis of the photopic luminosity function which yields only three basic factors.

Spatial Discriminations

Visual acuity and legibility.—Numerous factors influence speed and accuracy of reading letters, numerals, and symbols. Kuntz & Sleight (66) found

that the optimal ratios of height to width for black numerals on white ground and for white numerals on black ground fall on a curve which is practically flat (optimal) between 4:1 and 6:1, perhaps peaking at 4.5:1 on black background and at 5:1 on white background. These ratios held "pretty well" for legibility as a function of distance and for brightness levels of 10 ft. L. and lower. Numerals can be ordered from best to worst as follows: 1, 7, 0, 4, 3, 2, 9, 6, 5, and 8. The numeral 1 was legible at 1.6 m., while the 8 was legible at 0.9 m. with the others falling in between. The authors suggest that some of the numerals should be changed in form to improve their legibility. Berger (8) emphasizes the complexity of the conditions affecting legibility of symbols. He investigated the effect of height-width relations on legibility, using distance as the criterion. Since he used the numeral 5, squares of different areas, and the letter E, Berger's results cannot be directly compared with those of Kuntz & Sleight, but the conclusion that legibility increases with increasing height (within limits) holds for both studies. The effect of color of background and illuminant on legibility of Snellen letters is reported by Gilbert (29).

A somewhat different aspect of the problem was investigated by Davage & Sumner (21) who tested the effect of isolation on the legibility of Snellen letters. The Snellen chart has 61 letters and is ordinarily read line by line. Fifty subjects, divided into subgroups to balance practice and fatigue effects, read the chart under the following conditions: isolated letters obtained by covering the material with a gray cardboard with a viewing slot, nonisolated, monocularly, and binocularly. The score was the number of letters correctly read. The increase in correct reports of isolated over the nonisolated conditions was 20.4 per cent for the left eye alone, 22.5 per cent for the right eye alone, and 15.4 per cent for both eyes, all significant at the 1 per cent confidence level. The lower value for binocular vision should not be taken to mean that monocular vision was superior to binocular for such was not actually the case. On the average, subjects read seven more letters using both eyes than when using only one, but the percentage improvement was greater for the monocular condition.

Two studies on the relation of vernier to depth discrimination serve to bridge the gap between two and three dimensional seeing. In the first of these studies, Berry, Riggs & Duncan (9) studied the effect of field brightness on vernier and real depth discriminations. In the vernier situation, subjects saw two vertical lines in the same plane and reported whether the variable was displaced to the right or left; in the real depth situation, subjects had to tell if the variable was in back or in front of the standard. The brightness of the field was varied from 192 to 0.07 mL. At lowest levels of illumination, both vernier and real depth discrimination are limited by the failure of the eye to resolve the test objects, but at higher levels, other differential factors (which are not discussed) come into operation. In the second study, Berry, Riggs & Richards (10) studied the effect of angular width of the test objects on vernier and real depth discrimination, using essentially the same methods of presentation as in the study just referred to, but this time the width of the

rods was varied from 26.8″ to 424″. In neither type of discrimination was the width of the rods of significance, showing that it is the edge, not the area of the stimulus that determines its visibility. For all widths of rods, vernier acuity was better than depth acuity, the mean angular subtense being 3″ for the former and about 4″ for the latter.

The value of 3 to 4″ just given as the resolving power of the eye is about one-sixth to one-eighth of the average diameter of the cones and raises again the long-standing question as to how acuity can be so far under the diameter of the elements comprising the retinal mosaic mediating perception of two-ness. One suggestion, at present incapable of proof or disproof, has been that the cones are intertwined in such a way that the series of points forming the lines used in studies of acuity stimulate different sets of cones. Another, more plausible and testable hypothesis is that slight eye tremors cause the image of the test object to move across the retina, thereby furnishing differential stimulation either to the same column of cones or to contiguous ones. These involuntary motions of the eye, called physiological nystagmus, were investigated by Ratliff & Riggs (95) using monocular fixation. Photographic records were made of the eye fixating either a black point of 3.45′ on a 10° white background having a brightness of 60 ft.L. or a cross of 1.2′ with background brightness reduced to 45 ft.L. Five types of eye movement were recorded: (*a*) small, rapid motions, 30 to 70 c.p.s. and of 17.5 sec. extent, (*b*) large, slow motions, 2 to 5 c.p.s. and from 1 to 5′ of arc, (*c*) slow drifts on which the faster motions are superimposed which are usually no more than 5′ in extent, (*d*) rapid jerks at irregular intervals, ranging from 2.2 to 25.8′ of arc, and (*e*) total movement of all the preceding amounting to less than 10′ of arc in 3 to 4 sec. There were no significant differences between the point and the cross. The authors conclude that the retinal image remains on a single receptor no more than a few hundredths of a second, though it may remain on the most sensitive area of the fovea within a 2 to 3 sec. interval. The problem with which we began, a possible explanation of the remarkably low value of the acuity threshold, still remains to be explained. This study furnishes a necessary beginning toward the solution of the role of eye movements in acuity and other visual functions.

Figural aftereffects.—Later investigations of prolonged inspection of figures have revealed many new effects both during and after fixation. Recent studies tend to emphasize differences between the original Köhler-Wallach observations and later findings with the result that the brain mechanisms postulated by Köhler & Wallach for their more limited observations have come under fire. Köhler & Fishback (64) advance arguments supporting the theory that destruction of the Müller-Lyer illusion with repeated exposures is a satiation effect rather than a learning phenomenon. Destruction of the Müller-Lyer illusion is impeded by a long series of observations but is accelerated by long rest periods, a fact pointing to satiation, according to these writers. Similar phenomena in memory and learning may be partly caused by satiation, they maintain, though no proof can be given of this thesis at present. Hence, in advancing this theory, the authors are properly

XUM

cautious as they are aware of the slim evidence on which it is based.

The assumption made in the Köhler-Wallach theory that satiation is non-directional was tested by Hochberg (44). He inferred from it that black figure on white ground should give essentially identical results as white figure on black ground. Subjects fixated either a black cross on white ground or a white cross on black ground and then looked at a reversible black-white or white-black test figure. Hochberg found that if the test figure is like the satiating figure in color, it is seen less as figure in the reversible test figure than if it is of different color (really lightness). Hence, there are directional components in satiated brain fields, Hochberg concludes. Since lightness gradients are of prime importance in most visual phenomena, this finding of Hochberg's is not unexpected. The surprising thing is that this assumption forms part of the original theory.

Two studies, one by Prentice (93), the other by Hochberg & Bitterman (45), show that the figural aftereffect is determined by the actual angular size of stimulus, not by apparent size. This simplifies at least one aspect of the problem. But a study by Prentice & Beardslee (94) shows how complicated the effects can be from long fixation. Reverting to the original Gibson condition, they found that tilted lines tend to become more vertical with continued inspection. But after inspection of tilted lines, vertical lines tilt away from the originally fixated line. They conclude that the Gibson effects are not exactly the figural aftereffect of Köhler and Wallach since the Gibson effects are not dependent on either present or previous existence of other contours in the field. With this conclusion, there immediately follows another: there is no single theory at present for all the effects of satiation.

The possibility of retinal correlates for many, if not all, visual illusions and of configurational and satiation effects is opened up by the work of Motokawa (82). Using a technique apparently original with himself, Motokawa determined the electrical excitability of the retina following successive exposure to blue and white lights. An excitability curve having a maximum at 1.5 sec. after withdrawal of the white light coincides with the "crest time" obtained when yellow light alone is used as stimulus. This is taken as the physiological counterpart of successive color contrast. Spatial inductive effects were demonstrated on the same principle employed for temporal induction. It was assumed that stimulation of one part of the retina by yellow light induced a blue excitability in neighboring areas, a crest time of 1.5 sec. being expected in areas stimulated by blue light, followed by white stimulation. A modified procedure, based on this principle, was used to map the induced fields around various figures. The white test light was shifted little by little around the area exposed to stimulation by figures of various sizes and shapes, and the contrast effect was plotted as a function of distance from the contours both inside and outside the figure. For circle and triangle, the surrounding fields, mapped as lines of equi-induction, are circular and triangular, respectively, but for a square, the surrounding field tends to be cruciform. The field around a square inside a square framework is different from that of a square exposed alone. The maps of different figures and of

various combinations of figures, as well as of figures in various orientations, seem to be correlated with perceived phenomenological properties. Included in the discussion are the Benary paradoxical contrast effect, subjective gray dots, anisotropic effects, and satiation effects. Whether or not this study can be taken at face value must await duplication of Motokawa's results by others using his methods. If verified, his demonstrations of retinal isomorphisms with perceived data may open a new chapter in visual science.

The experiments reported by Lashley, Chow & Semmes (67) furnish the first direct test of Köhler's field theory of brain function since they were specifically designed to interfere with the electrical fields postulated in the theory. The deduction from field theory that unity and continuity of a figure are produced by a continuous region of high current density and that perceptual distance is increased by electrotonus or resistance to flow of current was tested by connecting remote parts of the visual cortex by a good conductor. A grid of conductors connecting various regions should produce multiple distortions in the perceptual field. Insertion of pins within an area should produce depolarization and distortion. Strips of gold foil were used to connect remote parts (lunate sulcus and the occipital pole), and gold pins were inserted in the macular representation to produce short-circuiting. Immature rhesus monkeys trained to differentiate visual objects were used as subjects. They had to choose one of two 8×8 sq. cm. metal plates covering food dishes. They differed in color, figureground relationships, and form. The tests showed no post-operative disturbances in behavior! Eye-movements were normal, no scotomata were detectable, distance perception and orientation were normal, there was no past-pointing, objects were recognized, and reaction time was normal. Hence, it is concluded there was no perceptual distortion to speak of. Space forbids more than this bare recital of the actual experiments. For the wealth of neuro-anatomical details which do not fit the theory of electrotonus, the reader is directed to the original report.

The lack of correspondence between Euclidean (physical) and visual space poses problems long investigated by physiologists and psychologists. Luneberg (71) has proposed a metric of binocular visual space designed to account for the indeterminacy of a single point in the dark (autokinetic sensation), sensed distances of points when several isolated points are viewed binocularly, the horopter curves of Helmholtz, the alley observations of Hillebrand and of Blumenfeld, and the constancy of perceived size. Since the mathematics in Luneberg's original treatment is too difficult for the psychologist with average training in mathematics, interested readers are referred to a paper by Hardy, Rand & Rittler (35) who have presented a simplified version in connection with their experimental tests of the theory. From them we learn that Luneberg's two constants σ and K are parameters varying from individual to individual, the first reflecting the sensitivity of the subject to depth as compared with lateral size, the second referring to the geometric character of his perceived space. Different values of K show whether a subject's perceived space is Euclidean, elliptic, or hyperbolic. Attempts to measure and classify subjects did not meet with success in the Hardy, Rand & Rittler experiments due to the fact that some subjects

could not follow instructions, some changed their criteria of parallel and equidistant from trial to trial when asked to set rows of lights parallel or equidistant, and finally, because of too many prior habits and associations. These investigators feel that the alley experiments do not furnish a good test of the theory (though Luneberg was a consultant on the project) and promise to try experiments outside the realm of the subjects' experience in the hope of obtaining more consistent results. However, Ogle, in his book referred to at the beginning of this chapter (87), cites evidence against the Luneberg theory based on experimental determinations of the binocular asymmetry of the two eyes. One cannot help but admire and encourage such attempts as Luneberg's because mathematical formulations provide the means for correcting and improving theories which are known to be imperfect idealizations to begin with.

Somewhat different in nature from the phenomena which Luneberg attempted to encompass in his theory are the distortions produced in complicated figures by including the Müller-Lyer figure in them as parts, as shown in a study by Rubin (97). Thus, if half of one side of a triangle is composed of the Müller-Lyer figure with arrows pointing in and the other side with arrows pointing out, the first side is contracted, the opposite side is expanded, and the figure as a whole undergoes distortion. Not only are the sides of the figure unequal, but the enclosed area also undergoes deformation. Rubin refers to the distorted figures as non-Euclidean because he regards them as incompatible with Euclidean geometry.

It is known that reduction of the visual field results in decrease of the apparent size of objects. Lichten & Lurie (69) have shown that perceived size follows the law of visual angle strictly when all extraneous light patterns are excluded and only the standard and variable stimuli are visible.

The old problem of "goodness of form" has again received attention in several studies, one of which by Casperson (14) concerns the discriminability of ellipse, rectangle, triangle, diamond, cross, and star, each presented in five sizes designed to keep areas, maximum dimensions, and perimeters as equal as possible. Discriminability of forms can be evaluated in a variety of ways so that the Gestalt criteria of goodness of figure in terms of simplicity and the like cannot be applied, a conclusion reached by Helson & Fehrer, as they point out, nearly 20 years ago. Circle and ellipse are in general not easy to identify. Casperson concludes that "increasing maximum dimensions and perimeter or the complexity of a form increases the probability of its being seen" (14, p. 679).

Quite a different test of the discriminability of squares, rectangles, circles, and ellipses was made by Sleight & Mowbray (104) who presented these figures on a plotting board and required their subjects to identify and locate them as rapidly as possible. Beginning with a one-inch square, rectangles were made by varying the square in one dimension in 0.078 in. steps or 7.8 per cent for each step. Similarly, ellipses were made by increasing the diameter of a circle 7.8 per cent for each step. The threshold for rectangles to be correctly identified 50 per cent of the time was between 1 $\frac{5}{64}$ and 1 $\frac{12}{64}$ in. or a difference of 14 per cent between sides. The ellipses required a

change of only 12 per cent to be perceived as such. The small difference between rectangles and ellipses was highly significant. One interesting finding was that rectangles were called squares more often in the vertical orientation than in the horizontal, showing that the horizontal lines were judged longer than the vertical, a finding not in accord with the horizontal-vertical illusion. The ellipses showed no such orientation effects.

Size constancy, distance, and depth.—The interrelationships of perceived visual size, distance, and depth are complicated and give rise to some paradoxical effects. Thus, Ittelson & Ames (51) varied the size of a playing card held at constant distance from subjects who reported the larger sizes as nearer and the smaller as farther away in space. The authors assume that the changes in accommodation and convergence, induced by change in apparent distance, may also be of importance in everyday life and in clinical practice. This is denied by Hofstetter (46) who points out that the convergence changes in their study are to be expected from changes in accommodation regardless of perceived distance of the object of regard, apparent size, or viewing distance. The phenomenon is well-known in clinical practice and is taken into account in optometry. In a second paper by Ittelson (50), using essentially the same technique as in the first, the conclusion is reached that "the object is localized by *O* at the point at which an object of physical size equal to the assumed size would have to be placed in order to produce the given retinal size" (50, p. 66). For criticism of the use of past experience in explanation of sensory phenomena, the reader is referred to an excellent article by Pratt (92). Theories and explanations based on limited observations involving no systematic variations of conditions and no measurements revealing functional relations do not materially advance our knowledge of phenomena.

An attempt to settle the question of whether light and shadow relations determine perceived depth as a learned or innate ability was made in a study by Hess (42). Two groups of chicks were reared with light from above and with light from below. If light-shadow relations are learned, the responses to depth should be different in the two groups when presented with photographs of grains differently illuminated from what they were accustomed to. The results showed that the chicks pecked predominantly at photographs of grains illuminated in the same manner as in the original training. Clear-cut as this experiment is in its design and controls, it would have contained more implications for the general problem had some independent criterion of depth been available. All that can be said from this study is that the chicks showed that their conditioning was effective!

That size constancy appears early in humans was again demonstrated by Dukes (23) who studied one six-year-old child as he noted the sizes of common objects in his environment and compared them with other objects. It would be profitable to see how children behave when the various cues are not too well-defined, as in the Ittelson & Ames experiments. Would they solve their problem in accordance with the equation relating size of object, size of retinal image, and distance of the eye from the object, as Schlosberg (101) believes adults do when indefinite or contradictory spatial cues are

XUM

present? Of course Schlosberg does not mean there is a conscious solving of an algebraic equation in such situations. Young children innocent of mathematics might still behave as adults do. The question is, how young do they begin to do so?

Perceived movement.—Interaction or field effects are also found in the perception of movement or velocity as pointed out by Johansson (54). When a kymograph is directly fixated, it appears to move more slowly than if the eyes are fixed on a point at rest near the moving object (the Auberg-Fleischl paradox). Johansson found that a second object in the field influences the perceived velocity of another object whether the first be stationary or moving. Perceptual velocity depends upon the interplay of spatial and temporal factors, but whenever the two are in conflict, it is invariably the temporal factor which primarily determines the perceived velocity.

Complex Visual Functions

Efficiency and comfort of vision.—One of the most controversial subjects in the field of human behavior concerns the effect of various working conditions on vision. Visual responses of various sorts (blink rate, eye movements, speed and accuracy of reading) have been used as indices of visual and even general bodily fatigue and various bodily responses (blink rate, heart action, muscle potentials) have been employed as indices of how well the eyes were performing. One of the most widely contested measures, blink rate, was again studied by Wood & Bitterman (118) who ask if frequency of blinking is a valid index of effort in visual work. Using Tinker's Speed of Reading Test, the subjects were instructed to work fast or slowly. A significantly greater rate of blinking was found during slow work than during fast. Contrary to the classical assertion of Luckiesh that frequency of blinking is directly related to effort, these findings indicate that blinking is inversely correlated with performance. In any case, as these authors point out, blinking is not a very sensitive or reliable index of visual effort.

In another study closely related to the one just discussed, Ryan, Cottrell & Bitterman (98) measured muscular tension as an index of effort under conditions of glare versus comfortable visual conditions. Subjects read material presented at controlled rates. Four areas of the body showed significant increments in muscle potentials under glare conditions. While the group averages showed significant differences, some individual results did not. To account for the individual differences, the authors point out that further research is necessary. A criticism of this study by one of the leaders in the field of muscular action potential studies [Jacobson (52)] does not seem to this reviewer to change the conclusions drawn by these investigators, especially in view of their reply in the same number of the journal.

Several studies have appeared comparing the effect on visual performance of fluorescent light with and without ultraviolet radiation and of daylight versus chromatic illumination. Sexton, Malone & Farnsworth (102) studied acuity and dark adaptation in relation to exposure to fluorescent light with and without ultraviolet components in the radiation. After adapting to fluorescent light and to ultraviolet shielded fluorescent light, subjects

showed no differences in their dark adaptation curves, measured for 4 hr. at ½ hr. intervals. Neither were any differences in visual acuity found after pre-adaptation to and working in shielded fluorescent lights as compared with unshielded radiation. The fluorescent lights employed did not yield sufficient ultraviolet to give measureable efforts. Similarly, a study by Gray & Prevetta (30) failed to reveal any significant differences between fluorescent and daylight illuminants in the case of subjects working for 2 hr. in each, though there was some loss in some functions at the end of the test periods for both types of illumination. Equivalent to a change in illumination, but possibly introducing refractive effects also, is vision with sun glasses of various types and grades. Peckham (89) tested visual acuity with and without glasses by means of charts containing specially devised Snellen letters. While acuity with any type of sun glass is lower than without, due to reduction in brightness of the field, contrary to popular belief, the more expensive sun glasses gave no better results than the poorest even though the price ratio was 20:1!

Guth (32) points out that while quality of lighting may have little effect on visibility (acuity) it may affect comfort considerably. The importance of light sources outside the direct line of vision is also stressed by him. He investigated the effect of brightness, size and position of the light source or sources in the visual field, and the level of adaptation of the eyes on comfort. Subjects sat at the center of an 80-inch sphere with a source of light at the center concealed from view. When the effect of multiple sources was in question, other lights were exposed in various parts of the field at various intensities. The illuminances studied varied from 0 to 30,000 ft.L. and were exposed for only 1 sec. at 10 sec. intervals. The subjects adjusted the sources until they were judged to be at the borderline between comfort and discomfort (BCD vision). The sources varied in size from 0.46 to 16 in., and at 40 in. from the eyes, were from 0.0001 to 0.126 steradians. The following equation for comfortable vision of a source summarizes an extensive series of observations:

$$B = 108PF^{0.44}(Q^{-0.51} - 1.28)$$

where B is comfortable vision, P represents the position index of the source with respect to the line of sight taken as base, F is the field brightness, and Q is the size of the source. The equation shows there is an inverse relation between size of source and BCD brightness. A method is also given for combining several sources in the field so that they can be treated as a single source. While this study seems at first sight to be a study in illuminating engineering, it is of importance also to the psychologist because it attempts to set up criteria for comfortable vision, it takes the adaptation of the eye into account, and it yields quantitative data regarding visual functioning under systematically varied field conditions.

In closing this discussion of factors affecting efficiency and comfort of seeing, it should be pointed out that so far as the writer is aware, no long-time aftereffects of the numerous factors studied have been reported. Con-

clusions from laboratory studies wherein various conditions fail to yield measurable differences in vision are unsafe if taken to mean that people can be exposed to these conditions day after day, year after year without final harmful effects. The eye is a peculiar organ. It complains very little, if at all, while it is being abused. Only months or years later after continued exposure to bad conditions do our eyes tell us what they have undergone. What the effects of exposure to fluorescent light from ordinary installations and from television screens (which also give off some soft x-radiation) will be on the coming generation when it reaches middle and old age no one can at present prophesy, at least not on the basis of studies made in the laboratory for a total of a few hours at most. Let us, therefore, be wary in our conclusions from short-time exposures and investigations to long-time exposures and possible long-time aftereffects. With quick conversion of scientific discoveries into practical gadgets, especially in the potentially dangerous area of the high energies of short-wave radiation, the possible dangers to vision are legion because they may not be detected until after the lapse of years.

Equilibration and vision.—It is well-known that bodily orientation depends upon vision and that exclusion or disturbance of visual data can interfere with maintenance of normal position and locomotion. Wapner & Witkin (116) investigated four conditions of the visual field as related to body-balance: (*a*) fully illuminated room with a frame completely visible, (*b*) limited visual field in which the subject could see only the upright luminescent frame and a fixation point, (*c*) absence of visual field, and (*d*) unstable visual field in which the luminescent frame in the darkened room was rocked to and fro. Steadiness and balance were found to grow poorer as the visual field was weakened, eliminated, and made unstable. Passey (88), however, minimizes the role of visual factors in the determination of the vertical. But an even more striking, because unexpected result, was the sex difference found by Wapner & Witkin: women were more strongly affected by the reduction of the visual field than men were, showing a greater dependence than men on the visual field in maintenance of body balance and, as previously shown, in adjusting rods to the vertical and horizontal positions.

More drastic changes in the visual field were made by Sperry (107) who rotated the eyeball in teleost fishes and also ablated the forebrain, optic tectum, cerebellum, and/or the otic labyrinth to determine the role of each. Surgical rotation of eyeball, with the other covered or removed, caused the fishes to circle without preference as to direction of circling. Neither bilateral ablation of the forebrain nor removal of the cerebellum made any difference in the circling motions, but excision of either or both optic lobes or of the right lobe alone abolished the circling response in cases where the left eye had been rotated. Removal of the labyrinths in otherwise normal fishes caused severe motor incoordination and disturbance in equilibrium. The survival of circling after bilateral labyrinthectomy in fishes with rotated eye was surprising because it seems to make circling a purely optic effect. Sperry, however, believes that it is unlikely that circling is due to purely

altered retinal stimulation and points out that the inversion caused by rotation of the eye is really with reference to the body and its movement. A given retinal pattern may or may not arouse spinning reaction, depending upon the direction of the animal's movement accompanying the total retinal influx. After extirpation of the labyrinths, kinesthetic impulses from other sources may have been responsible for the circling, but it is still an open question as to why labyrinthectomy did not cause greater effects. Sperry postulates a central kinetic factor arising as part of the excitation pattern of the overt movement which serves as a basic background for all space perception, whether it be principally visual, auditory, or cutaneous. Thus, a neuro-anatomical study leads to conclusions similar to conclusions from behavioral studies of space perception and judgment.

Four more studies dealing with different types of visual activities are of importance for our survey. Witkin (117), in a study of time taken to identify simple Gottschaldt figures embedded in complex figures, found that it required a total of 15 min. 24 sec. for men and 23 min. 18 sec. for women to do the series. As reported above, Witkin had previously found that women were generally more strongly influenced than men by the structure and direction of the prevailing visual field when perceiving the direction of an item within the field. So the present finding fits with the earlier one. But the question remains "To what is this sex difference due?" That it is not due to sex, one feels quite sure. What factors in upbringing can account for it then?

Another study concerns marked differences in the ability of the blind to perceive obstacles. Worchel, Mauney & Andrew (119), in carefully controlled experiments, found that of 34 totally blind subjects, seven did not possess any obstacle sense at all. Previous studies have indicated that the blind use auditory cues in the detection of objects, and it would be interesting to see what training could do for these seven who have apparently not learned to utilize them.

Thurstone (112) concludes there are seven primary abilities in visual thinking. They are: (*a*) perceptual speed, (*b*) closure ability, (*c*) closure ability against distraction, (*d*) visual memory, and three space factors, (*e*) S_1, perception of identity of an object seen from different angles, (*f*) S_2, ability to imagine movement among parts of a configuration one is thinking about, and (*g*) S_3 space relations in which body orientation is part of the problem. Thurstone points out that these are independent abilities since excelling in one does not mean excelling in the others.

Only one study was found dealing with the esthetic aspect of vision, a study of color preferences by Buck (13). Studying color likes and dislikes of subjects viewing objects illuminated by various types of fluorescent light, Buck found so many factors entering in, such as order of presentation, illumination level, surroundings of the object, temperature, humidity, time of day, and general health of the subjects, that little is actually added to what is already known, or not known, about this most popular but least understood subject in the psychology of vision.

LITERATURE CITED

1. Adler, F. H., *Physiology of the Eye* (C. V. Mosby Co., St. Louis, 709 pp., 1950)
2. Asher, H., *Brit. J. Psychol.*, **40**, 187–94 (1950)
3. Attneave, F., *Am. J. Psychol.*, **63**, 516–56 (1950)
4. Attneave, F., and McReynolds, P., *Am. J. Psychol.*, **63**, 107–10 (1950)
5. Ballard, S. S., *J. Optical Soc. Am.*, **40**, 881–82 (1950)
6. Bartlett, N. R., *Ann. Rev. Psychol.*, **1**, 31–48 (1950)
7. Baumgardt, E., *Compt. rend.*, **230**, 2124–26 (1950)
8. Berger, C., *Acta Ophthalmol.*, **28**, 423–34 (1950)
9. Berry, R. N., Riggs, L. A., and Duncan, C. P., *J. Exptl. Psychol.*, **40**, 349–54 (1950)
10. Berry, R. N., Riggs, L. A., and Richards, W., *J. Exptl. Psychol.*, **40**, 520–22 (1950)
11. Bouman, M. A., *J. Optical Soc. Am.*, **40**, 825–32 (1950)
12. Bouman, M. A., and van der Velden, H. A., in *Documenta Ophthalmologica: Advances in Ophthalmology*, **3**, 324–27 (Fischer, F. P., Schaeffer, A. J., and Sorsby, A., Eds., Uitgeverij Dr. W. Junk, 'S-Gravenhage, Holland, 327 pp., 1949)
13. Buck, G. B., *Illum. Eng.*, **45**, 165–73 (1950)
14. Casperson, R. C., *J. Exptl. Psychol.*, **40**, 668–81 (1950)
15. Casperson, R. C., and Schlosberg, H., *J. Exptl. Psychol.*, **40**, 81–92 (1950)
16. Chang, H-T., and Kaada, B., *J. Neurophysiol.*, **13**, 305–18 (1950)
17. Chapanis, A., *Ann. Rev. Psychol.*, **2**, 45–64 (1951)
18. Chatham, G. N., *Am. J. Psychol.*, **63**, 612–15 (1950)
19. Condon, E. U., *Proc. Inst. Radio Engrs.*, **38**, 980–1002 (1950)
20. Crozier, W. J., *J. Gen. Physiol.*, **34**, 87–134 (1950)
21. Davage, R. H., and Sumner, F. C., *J. Psychol.*, **30**, 191–94 (1950)
22. Davidson, H. R., *J. Optical Soc. Am.*, **41**, 104–10 (1951)
23. Dukes, W. F., *Am. J. Psychol.*, **64**, 87–93 (1951)
24. Duncan, D. R., *J. Oil & Colour Chemists' Assoc.*, **32**, 296–321 (1949)
25. Evans, R. M., *J. Optical Soc. Am.*, **39**, 774–79 (1949)
26. Feldman, J. B., *Am. J. Ophthalmol.*, **33**, 777–85 (1950)
27. Gibson, J. J., *Am. J. Psychol.*, **63**, 367–84 (1950)
28. Gibson, J. J., *The Perception of the Visual World* (Houghton Mifflin Co., Boston, 235 pp., 1950)
29. Gilbert, W. M., *Am. J. Optometry*, **27**, 407–11 (1950)
30. Gray, J. S., and Prevetta, P., *J. Applied Psychol.*, **34**, 235–36 (1950)
31. Guilford, J. P., *Am. J. Psychol.*, **38**, 534–83 (1927)
32. Guth, S. K., *J. Optical Soc. Am.*, **41**, 235–44 (1951)
33. Hanes, R. M., *J. Exptl. Psychol.*, **40**, 650–54 (1950)
34. Hanes, R. M., *J. Optical Soc. Am.*, **41**, 28–31 (1951)
35. Hardy, L. H., Rand, G., and Rittler, M. C., *Arch. Ophthalmol. (Chicago)*, **45**, 53–63 (1951)
36. Hartridge, H., *Practitioner*, **163**, 182–86 (1949)
37. Hartridge, H., and Weale, R., *Nature*, **164**, 999–1000 (1949)
37a. Hecht, S., Shlaer, S., and Pirenne, M. H., *Science*, **93**, 585 (1941)
38. Helson, H., Ed., *Theoretical Foundations of Psychology* (D. Van Nostrand Co., Inc., New York, 787 pp., 1951)

XUM

38a. Helson, H. (Unpublished data)
39. Helson, H., and Guilford, J. P., *J. Gen. Psychol.*, **9,** 58–75 (1933)
40. Helson, H., and Michels, W. C., *J. Optical Soc. Am.*, **38,** 1025–32 (1948)
41. Helson, H., Judd, D. B., and Warren, M., *Illum. Eng.* (In press)
42. Hess, E. H., *J. Comp. Physiol. Psychol.*, **43,** 112–22 (1950)
43. Hess, E. H., *Am. J. Psychol.*, **63,** 259–60 (1950)
44. Hochberg, J. E., *J. Exptl. Psychol.*, **40,** 682–86 (1950)
45. Hochberg, J. E., and Bitterman, M. E., *Am. J. Psychol.*, **64,** 99–102 (1951)
46. Hofstetter, H. W., *J. Psychol.*, **30,** 393–94 (1950)
47. Hunt, R. W. G., *Proc. Phys. Soc.* (*London*) [B]**62,** 203–6 (1949)
48. Hunt, R. W. G., *J. Optical Soc. Am.*, **40,** 362–70 (1950)
49. Hurvich, L. M., and Jameson, D., *A Psychophysical Specification of White, Eastman Kodak Co., Color Control Dept. Rept.*, No. A-82, 78 pp. (1951)
50. Ittelson, W. H., *Am. J. Psychol.*, **64,** 54–67 (1951)
51. Ittelson, W. H., and Ames, A., Jr., *J. Psychol.*, **30,** 43–62 (1950)
52. Jacobson, E., *Am. J. Psychol.*, **64,** 112–17 (1951)
53. Jacobson, H., *Science*, **113,** 292–93 (1951)
54. Johansson, G., *Acta Psychologica*, **7,** 25–79 (1950)
55. Jones, F. N., and Jones, M. H., *Am. J. Psychol.*, **63,** 206–13 (1950)
56. Judd, D. B., *U. S. Dept. Commerce Research Paper, RP 1922*, **41,** 247–271 (1948)
57. Judd, D. B., *J. Optical Soc. Am.*, **39,** 945–50 (1949)
58. Judd, D. B., *U. S. Dept. Commerce Research Paper, RP 1946*, **42,** 1–16 (1949)
59. Judd, D. B., *Colorimetry, Natl. Bur. Standards* (*U. S.*) *Circ.*, No. 478, 56 pp. (1950)
60. Judd, D. B., *Color in Medical Physics*, 1149–61 (Glaser, O., Ed., Year Book Publishers, Inc., Chicago, 1950)
61. Judd, D. B., Plaza, L., and Farnsworth, D., *J. Optical Soc. Am.*, **40,** 833–41 (1950)
62. Judd, D. B., *U. S. Dept. Commerce Research Paper, RP 2053*, **44,** 1–8 (1950)
63. Knox, G. W., and Ellerbrock, V. J., *Light, Vision and Lenses* (Scioto Printing Co., Columbus, 350 pp., 1950)
64. Köhler, W., and Fishback, j., *J. Exptl. Psychol.*, **40,** 398–410 (1950)
65. Koomen, M., Scolnik, R., and Tousey, R., *J. Optical Soc. Am.*, **41,** 80–90 (1951)
66. Kuntz, J. E., and Sleight, R. B., *Am. J. Psychol.*, **63,** 567–75 (1950)
67. Lashley, K. S., Chow, K. L., and Semmes, J., *Psychol. Rev.*, **58,** 123–46 (1951)
68. Lehmann, H., *Science*, **112,** 199–201 (1950)
69. Lichten, W., and Lurie, S., *Am. J. Psychol.*, **63,** 280–82 (1950)
70. Low, F. N., *Arch. Ophthalmol.* (*Chicago*), **45,** 80–99 (1951)
71. Luneberg, R. K., *J. Optical Soc. Am.*, **40,** 627–42 (1950)
72. MacAdam, D. L., *Documenta Ophthalmologica: Advances in Ophthalmology*, **3,** 214–39 (Fischer, F. P., Schaeffer, A. J., and Sorsby, A., Eds., Uitgeverij, Dr. W. Junk, 'S-Gravenhage, Holland, 327 pp., 1949)
73. MacAdam, D. L., *J. Optical Soc. Am.*, **40,** 589–95 (1950)
74. Mann, I., *The Development of the Human Eye* (Brit. Med. Assoc., London, 312 pp., 1949)
75. Marg, E., *Arch. Ophthalmol.* (*Chicago*), **45,** 169–85 (1951)
76. Michels, W. C., and Helson, H., *Am. J. Psychol.*, **62,** 355–68 (1949)
77. Miles, P. W., *Am. J. Ophthalmol.*, **33,** 769–73 (1950)

XUM

78. Millard, E. B., and McCann, W. S., *J. Applied Physiol.*, **1,** 807–10 (1949)
79. Mitchell, R. T., Morris, A., and Dimmick, F. L., *Report No. 166*, **9,** 258–77 (Naval Med. Research Lab., U. S. Submarine Base, New London, Conn., 1950)
80. Mote, F. A., Riopelle, A. J., and Meyer, D. R., *J. Optical Soc. Am.*, **40,** 584–88 (1950)
81. Mote, F. A., and Riopelle, A. J., *J. Optical Soc. Am.*, **41,** 120–24 (1951)
82. Motokawa, K., *J. Neurophysiol.*, **13,** 413–26 (1950)
83. Mueller, C. G., *Am. J. Psychol.*, **63,** 92–100 (1950)
84. Mueller, C. G., *J. Gen. Physiol.*, **34,** 463–74 (1951)
85. Nash, M. C., *Am. J. Psychol.*, **63,** 214–20 (1950)
86. Newhall, S. M., *Am. J. Psychol.*, **63,** 221–28 (1950)
87. Ogle, K. N., *Researches in Binocular Vision* (W. B. Saunders Co., Philadelphia, 345 pp., 1950)
88. Passey, G. E., *J. Exptl. Psychol.*, **40,** 738–45 (1950)
89. Peckham, R. H., *J. Optical Soc. Am.*, **41,** 23–27 (1951)
90. Pickford, R. W., *Brit. J. Psychol., Gen. Sect.*, **41,** 52–62 (1950)
91. Pickford, R. W., *Individual Differences in Colour Vision* (Routledge & Kegan Paul, Ltd., London, 386 pp., 1951)
92. Pratt, C. C., *J. Psychol.*, **30,** 85–107 (1950)
93. Prentice, W. C. H., *Am. J. Psychol.*, **63,** 589–93 (1950)
94. Prentice, W. C. H., and Beardslee, D. C., *J. Exptl. Psychol.*, **40,** 355–64 (1950)
95. Ratliff, F., and Riggs, L. A., *J. Exptl. Psychol.*, **40,** 687–701 (1950)
96. Rose, A., *J. Optical Soc. Am.*, **41,** 210–11 (1951)
97. Rubin, E., *Acta Psychologica*, **7,** 365–87 (1950)
98. Ryan, T. A., Cottrell, C. L., and Bitterman, M. E., *Am. J. Psychol.*, **63,** 317–41 (1950)
99. Saltzman, D. C., and Garner, W. R., *J. Psychol.*, **29,** 453–67 (1950)
100. Scheidemann, N. V., *Am. J. Psychol.*, **63,** 441–45 (1950)
101. Schlosberg, H., *J. Exptl. Psychol.*, **57,** 314–17 (1950)
102. Sexton, M., Malone, F., and Farnsworth, D., *Report No. 169*, **9,** 301–17 (Naval Med. Research Lab., U. S. Submarine Base, New London, Conn., 1950)
103. Sinden, R. H., *J. Optical Soc. Am.*, **40,** 647–52 (1950)
104. Sleight, R. B., and Mowbray, G. H., *J. Psychol.*, **31,** 121–27 (1951)
105. Sloan, L. L., *Am. J. Ophthalmol.*, **33,** 1077–89 (1950)
106. Sperling, H. G., and Farnsworth, D., *Report No. 154*, **9,** 54–89 (Naval Med. Research Lab., U. S. Submarine Base, New London, Conn., 1950)
107. Sperry, R. W., *J. Comp. Physiol. Psychol.*, **43,** 482–99 (1950)
108. Stevens, S. S., Ed., *Handbook of Experimental Psychology* (John Wiley & Sons, Inc., New York, 1436 pp., 1951)
109. Tanner, W. P., Jr., *Science*, **112,** 201–3 (1950)
110. Thomas, E. B., *Arch. Ophthalmol. (Chicago)*, **43,** 729–41 (1950)
111. Thomson, L. C., *Proc. Phys. Soc. (London)* [B]**62,** 787–92 (1949)
112. Thurstone, L. L., *Proc. Am. Phil. Soc.*, **94,** 517–21 (1950)
113. Wald, G., *Science*, **113,** 287–91 (1951)
114. Wald, G., and Brown, P. K., *Proc. Natl. Acad. Sci.*, **36,** 84 (1950)
115. Wald, G., and Hubbard, R., *Proc. Natl. Acad. Sci.*, **36,** 92 (1950)
116. Wapner, S., and Witkin, H. A., *Am. J. Psychol.*, **63,** 385–408 (1950)

XUM

117. Witkin, H. A., *J. Personality*, **19**, 1–15 (1950)
118. Wood, C. L., and Bitterman, M. E., *Am. J. Psychol.*, **63**, 584–87 (1950)
119. Worchel, P., Mauney, J., and Andrew, J. G., *J. Exptl. Psychol.*, **40**, 746–55 (1950)
120. Wright, E. D., *Documenta Ophthalmologica: Advances in Ophthalmology*, **3**, 10–23 (Fischer, F. P., Schaeffer, A. J., and Sorsby, A., Eds., Uitgeverji Dr. W. Junk, 'S-Gravenhage, Holland, 327 pp., 1949)

XUM

HEARING[1,2]

By Wendell R. Garner

Psychological Laboratory, The Johns Hopkins University, Baltimore, Maryland

If the progress of science in a particular area can be measured by the need for and the existence of large specialized bibliographies, then certainly audition is making progress. Miller, Rosenblith, Galambos & Hirsh (59), at the Harvard Psycho-Acoustic Laboratory, have collaborated in producing an extensive bibliography in audition. A two-volume work, it contains 5,500 entries and covers the period from 1938 to 1949 thoroughly and the prior period somewhat less thoroughly. The main part of the bibliography lists references by author, but at the end there is a detailed topical index with major categories of anatomy, physiology, psychology, deafness, and theory. This reference work has filled a very real need for psychologists and others working in the area of hearing and will continue to be used for years to come. A more specialized bibliography of 310 references has been compiled by Wing (84) on the microphonic action of the cochlea.

Another general reference work has been published in the past year by Kryter (49) on the effects of noise on man. The monograph is divided into three main sections. The first section deals with the effects of noise on general behavior and leads to the usual conclusion that experiments seldom show any direct effect of noise on mental or physical work efficiency. The second section, which deals with the deafening effects of noise, concludes that high noise levels certainly cause temporary hearing loss and quite probably some permanent hearing loss. The third section deals with the effect of noise on speech communication. Here the author has somewhat more clear-cut conclusions, for the easiest demonstrable effect of noise is on the ability of people to hear each other talk.

One more point of general interest is that information theory and some of the mathematical techniques used in its application will probably become more prevalent in the future. Already, Jacobson (44) has computed that the ear is capable of handling 8,000 bits of information per second. This type of analysis is not, however, a good example of the possible utility of information theory for problems of audition. His analysis is based on difference limens and some very simple assumptions about the lack of interaction effects in auditory perception. While such computations are perhaps of passing interest, it is doubtful that they make any real contribution to our understanding of the hearing process.

[1] The survey of the literature to which this review pertains was completed in May, 1951.

[2] This chapter was prepared with the assistance of Contract N5-ori-166, Task Order I, between Special Devices Center, Office of Naval Research and The Johns Hopkins University. This is Report No. 166-I-132, Project Designation No. NR-784-001, under that contract.

Electrophysiology of the Cochlea

Békésy's interest has shifted in the past year from the mechanics of the cochlea to some of the electrical properties of the cochlea. In a beginning investigation, he (6) has measured some of the simpler electrical properties of the cochlea. The maximum longitudinal electrical impedance turns out to be on the order of 30,000 ohms, while the impedance of the cochlear tube itself is between 1,000 and 1,500 ohms. Stating the figures so simply does not adequately portray the fact that an electrical circuit analogy of the cochlea is relatively complicated. Such measurements as he has made will be useful in determining energy losses in the cochlea, the manner of transmission of microphonic voltages to the round window, etc.

In order to determine the nature of the cochlear microphonic, Békésy (7) set the basilar membrane into motion by touching it with a small vibrating instrument. By using a trapezoidal wave form, he was able to show that the microphonic is a function of displacement, not of the velocity of movement. If it had been a function of velocity, the microphonic would have been a square wave with its maximum during the sloping period of the trapezoid. Since, however, the microphonic followed the trapezoidal wave form exactly, his conclusion is quite clear.

Several papers on electrical activity in the cochlea have come from the Central Institute for the Deaf. Fernández *et al.* (22) damaged the cochlea at various points by drilling small holes in the bony cochlea and passing various types of electrical current through restricted segments of the cochlea. The resulting damage to the cochlea (due both to heating effects and bubbles formed by electrolysis) raised the threshold for the microphonic, and the results with various frequencies showed a rough agreement with place theory.

By using a special feedback technique to attenuate the microphonic, Davis, Gernandt & Riesco-MacClure (13) were able to study the neural action potential with increased accuracy; they found that below 1,000 c.p.s., the threshold for the action potential decreased linearly with an increase in frequency. When the action potential was suppressed with quinine, it was found that the cochlear microphonic was virtually independent of frequency up to 5,000 c.p.s. Further evidence that the action potential as recorded at the round window and the cochlear microphonic are quite distinct electrical processes has been provided by Kahana, Rosenblith & Galambos (45). They worked with the hamster, since its body temperature can be changed over a wide range without killing the animal. Their experiments showed that the microphonic and the action potential behave quite differently with respect to the effect of body temperature. The neural action potential shows a decreasing latency with increasing body temperature, while the microphonic shows no change in latency. Also, the neural response and the microphonic show different changes in amplitude with decreased body temperature. There thus seems to be little doubt that the neural action potential and the cochlear microphonic are different and are generated by different parts of the auditory system.

XUM

Finally, Davis, Fernández & McAuliffe (12) have identified in the cochlea a new kind of electrical activity which they call the summating potential. At low frequencies, the action potential is synchronized with the stimulating frequency. If a pulse of sine-wave energy at a high frequency is used, however, there is a unidirectional wave which follows roughly the form of the pulse. They list several ways in which this potential is different from the cochlear microphonic and the action potential, including differences in phase and decay rate, different conditions under which each is destroyed, and differences in polarization. It is their opinion that the summating potential is the actual neural stimulator. If these findings are verified in other experiments and in other laboratories, a new and important advance will have been made. However, because of the lack of confirmation from other experimenters and because of the complexity of the electronic techniques used in these experiments, it is perhaps best to withhold judgment about the value of this discovery.

Audiometry

A large-scale hearing survey was conducted by the Naval Electronics Laboratory at the San Diego County Fair, where absolute and masked theshold data were obtained from 3,666 people. In many respects, the data simply confirm those from previous surveys. In one analysis, Webster, Himes & Lichtenstein (77) showed that hearing loss is positively related to awareness of hearing loss, but that of the frequencies they tested, the relation is strongest for a frequency of 1,760 c.p.s. It is in this frequency range that the most important speech frequencies occur. [Fletcher (25) found, in this connection, that an average of the two lowest hearing losses for frequencies of 500, 1,000, and 2,000 c.p.s. gives an estimate of the hearing loss for speech with a root mean square difference of 5.1 db. from directly determined hearing losses for speech.] Thus, it seems likely that awareness of hearing loss is most closely related to hearing loss for speech. Their other data on absolute thresholds agree quite well with those of Steinberg, Montgomery & Gardner (72) from the World's Fair.

It has frequently been assumed that deaf people can hear better, or certainly as well as normals, in noisy environments. The analysis of the masked threshold data by Webster, Lichtenstein & Gales (78) casts doubt on this assumption in showing that there is a marked positive correlation between the absolute and the masked threshold, with an average increase of 1 db. in the masked threshold for every 10 db. increase in the absolute threshold. Maré & Roesler (57) point out that occasionally people with conductive deafness may be able to hear better in noise than normals, but that people with perceptive deafness are more susceptible to masking than normals.

Absolute threshold measurement.—Attempts at fine precision in psychophysical measurement have usually been aimed at absolute threshold measurements, and this past year has been no exception. Myers & Harris (63) showed that there is little more variability in the absolute threshold from day to day or from week to week than from minute to minute. Munson &

XUM

Wiener (62), however, have shown that whatever the variability is, it could be smaller if measurement of the physical intensity is made at the entrance to the ear canal with a probe tube rather than at the receiver voltage. At least some of the variability is in the instruments, not in the observer. Furthermore, part of the observer variability is due to the effect of preceding items on judgments about the audibility of a tone. Schafer (70) found that either relatively weak or relatively strong items preceding the item in question decrease the probability of hearing by as much as 25 per cent. Although it is difficult to see how such precedence effects can be avoided, still it is probably a good idea to set up counterbalanced lists so as to cancel out any systematic effect in determining psychometric functions.

Hardy & Bordley (33) have continued their work on the use of the conditioned galvanic skin response (GSR) as an audiometric technique for use with children, cases of audiogenic deafness, malingering, and other cases where there may be communication difficulties. In one group of 34 children ranging in age from 4 to 18 years, the difference in threshold obtained with the conditioned GSR and the usual verbal techniques was as great as 20 db., with well over 50 per cent showing a difference as great as 5 db. The GSR (without conditioning) was used by Knapp & Gold (47) at an Army Hearing Rehabilitation Center, and they were able to obtain the GSR at some intensity in 88 per cent of the cases tested. They found the technique particularly useful in testing for psychosomatic deafness.

Loudness and Masking

The loudness scale.—Philosophical arguments about whether a psychological scale can have a magnitude in the physicist's sense of the word will probably continue for decades. Fortunately, those working in the area of hearing have not been bothered about these arguments and have gone ahead working with loudness and pitch scales as though these scales represented real magnitudes. This is fortunate because some of the better advances in integrating diversified material have been made through the intermediary of the loudness scale. Fletcher & Munson (26) first realized that the loudness of multicomponent tones could be determined only if the physical magnitudes of the component tones were converted into psychological magnitudes before addition. After addition, of course, the loudness could be reconverted to an equivalent physical measure, such as the intensity level of a 1,000 c.p.s. tone; but the actual addition had to be carried out with loudnesses, not the physical intensities. The technique they worked out for dealing with a series of discrete tones had its limitations, however, because if the different components were spaced too closely together, then psychological magnitudes would not add properly. Fletcher & Munson (27) later worked out a technique which made use of the masking audiogram to compute the loudness of complex sounds having continuous spectra. Recently, Beranek *et al.* (8) have shown that these continuous spectra sounds can be handled with simple addition of loudnesses if the continuous spectrum is subdivided into smaller bands of

frequencies between 300 and 600 mels wide. The average loudness levels in phons of each band are then determined, the loudnesses computed from these loudness levels, and these loudnesses added to give the total loudness of the complex sound.

In another application of the loudness scale, Stuntz (73) has been able to reconcile two seemingly conflicting sets of data. Previously, Eisenberg & Chinn (21) had shown that listeners prefer a narrow frequency range when listening to recorded music. Olson (65), on the other hand, had demonstrated that listeners prefer wide frequency bands. Since Olson had used acoustic, rather than electronic, filters, it was supposed that the difference lay in certain types of intermodulation distortion which can occur with electronic filters. Stuntz showed, however, that their results were in essential agreement if the frequency response curves were plotted in terms of loudness. His essential argument was that the frequency bandwidth as measured in physical terms may be quite misleading in showing what the psychological bandwidth is and that it is the latter which is critical.

Masking.—In the classic experiments of Wegel & Lane (79), the manner in which pure tones are masked by other pure tones was partially obscured because of beating between the masked tone and the fundamental and harmonics of the masking tone. In order to avoid these difficulties, Egan & Hake (18) have redone some of the masking functions by using a narrow band of noise as the masking stimulus. Under these conditions, the masking function is much smoother and shows much more masking when the masked frequency is at the middle of the band of the masking noise. Their computation of the size of the critical band for this experiment, however, indicates a somewhat smaller critical band than is indicated by Fletcher (24) and other experimenters using continuous noise spectra.

Schafer *et al.* (71) also studied the masking of pure tones by narrow bands of noise in an attempt to test the critical band concept directly. In order to get sharply defined bands of noise, they built up a noise spectrum by recording groups of pure tones and then playing the recordings back. With this technique, they were able to determine the masked threshold as a function of the width of the band and to determine that band width where no further increase in width would produce an increase in masking. They found that masking is achieved when the masked signal had a level 1 to 2 db. below the level of the masking noise. Thus, it would seem that the concept of the critical band as that band of frequencies whose energy will just equal the energy of a tone which the band will mask is not validated.

However, the concept of the critical band is probably being overworked when it is used for a direct test of this sort. The ability to produce a precisely defined band of noise physically is not the same as the ability to produce a precisely defined band of noise on the basilar membrane. In fact, about the only way in which noise energy can be conceived as a series of rectangular bands of energy is in the case of a relatively continuous spectrum of noise. In such a case, energy from each band will spill over into adjacent bands, but

energy from these adjacent bands will also spill over into the band in question, and everything will come out approximately even. When a narrow band of noise is used alone, however, much of the masking energy is lost, and it is to be expected that the threshold for a pure tone in the center of the band will be less than when a continuous band of noise is used. Thus, these results, which show the masked threshold for the pure tone to be lower (and thus the critical band to be smaller), do not seem at variance with a rational analysis of the problem of producing rectangular distributions of energy on the basilar membrane.

In another experiment, Egan & Meyer (20) demonstrated that the masking pattern produced by the narrow band of noise can either lower or increase the pitch of a partially masked tone. If the tone is higher in frequency than the center of the band, the pitch is raised. If the tone is lower, its pitch will be lowered. These results are explained in terms of residual loudness patterns. For example, the higher frequency tone has the lower part of its area of disturbance on the basilar membrane masked more than its upper part, and its residual loudness pattern will have a peak at a higher frequency. Their computations of this effect provide an excellent explanation of their results.

One further study on masking was conducted by Hirsh, Rosenblith & Ward (41). In this study, they masked clicks with bands of noise or pure tones. Frequencies in the range 1,000 to 3,000 c.p.s. are the most effective maskers. The relation between masking of clicks and the intensity of the masking signal is not a simple linear relation. There is a leveling off in the function between 40 and 70 db., indicating that a 40 db. signal masks a click about as well as a 70 db. signal. This is an interesting phenomenon, and a good explanation would be very welcome.

The method of best beats.—The method of best beats has long been a standard technique for determining the effective intensity of a tone when its intensity cannot be directly measured. With this method, a tone of known intensity is made to beat with the tone of unknown intensity, and the intensity of the known tone is varied until the best beats have been obtained. Egan and his collaborators have shown that this method gives a small enough standard deviation to be a reliable technique (76), that over a wide range of intensities there is a linear relation between the intensities of the two beating tones (46), and that a serious error occurs when the tone of unknown intensity is near its masked or absolute threshold (19).

In this latter case, the intensity will be estimated as too high by as much as 10 db. The authors show that the approximate size of the overestimation can be estimated from the range of audible beats—if the range is large, the overestimation is small, and vice versa. Their explanation of the effect is that the best-beat condition is determined by the range of loudnesses through which the combined tone goes. If one tone is near threshold, then the intensity which gives the best physical beat will make the combined tone go below threshold for part of each beat cycle. The best-beat condition is that which

allows the minimum intensity during the beating cycle to go just to, but not below, the threshold. With this rationale and with known thresholds, an exact expression can be worked out to predict the condition of best beat. This mathematical formulation predicts very accurately the results of their experiments.

Auditory Fatigue and Deafness

It has always been difficult to determine the extent to which exposure to intense sounds can produce temporary or permanent deafness because the only available data are usually obtained from industrial situations where poor experimental control has been the rule. On the other hand, experimenters have been reluctant to run the risk of intentionally producing permanent deafness for experimental purposes in humans. One set of experiments, however, was carried out by Davis *et al.* (14) during the late war on human subjects, and while the most severe possible situations were not used, still the conditions of stimulation were severe enough to allow some valid conclusions. In their experiments, the ears of 19 men were stimulated by frequencies of 500, 1,000, 2,000, or 4,000 c.p.s. at intensities of 110, 120, and 130 db. sound pressure level (SPL), for durations of 1 to 64 min. Their major conclusions can best be listed as follows:

(*a*) The higher frequencies of stimulation are the most effective in producing hearing loss, and the area of major loss is normally about half an octave above the frequency of stimulation. [In this connection Wheeler (82) found that thermal noise is much more effective in producing temporary deafness than simulated airplane noise, presumably because of the higher frequency components in the thermal noise.] (*b*) Hearing loss is greater for more intense stimulation. (*c*) Hearing loss develops most rapidly at first, and then more slowly; likewise, the most rapid recovery is early. In severe cases, however, as much as four or five days was necessary for complete recovery. (*d*) Hearing loss is not progressive with successive exposures. (*e*) The hearing loss is of a perceptive deafness type. When loudness balances are made, recruitment of loudness occurs. (*f*) Pitch distortion and diplacusis occur, with the pitch always shifting in an upward direction. This pitch shift may be as much as three-quarters of an octave.

In similar experiments, Hood (42) arrives at conclusions much like those stated above. In addition, he found that 95 db. SPL was a critical intensity for the production of hearing loss. In further experiments, Hood also determined the fatigue to a continuing tone, as measured by its apparent loudness. In these experiments, one ear was stimulated continuously, while the other ear received only an occasional comparison tone which was matched in loudness to the stimulating tone. Perstimulatory fatigue, as contrasted with poststimulatory fatigue, continues for as much as 3 min., with an equivalent intensity loss of as much as 35 db. The author points out that persons with perceptive or nerve deafness are unusually susceptible to both forms of auditory fatigue. It is interesting to note the similarity of the behavior of

XUM

perceptively deaf people in relation to their abnormal susceptibility both to auditory fatigue and to masking. Wilson (83) has also shown that there are wide individual differences in susceptibility to auditory fatigue. In a practical application of this type of research, Ogden (64) has shown that gunnery instructors can avoid the temporary hearing losses produced by constant listening to machine gun fire by wearing ear protectors.

Short Duration Auditory Fatigue

Harris, Rawnsley & Kelsey (36) and Harris (35) have used the term "short duration auditory fatigue" to describe an effect similar to the more severe stimulation or adaptation deafness. Whatever the name, the nature of the phenomenon is clear: for a short time after stimulation by a tone, the threshold for a tone of the same or similar frequency is elevated. Although the experimental paradigm is similar to that for stimulation deafness, because of the completely different orders of times and intensities, the effect should probably be considered as a continuation of masking, not as a deafness phenomenon. The distinctions between the two phenomena are not clear, however.

This effect has been used in the diagnosis of deafness by Gardner (31) and in the determination of loudness patterns by Munson & Gardner (61). The studies mentioned above are the most complete on the phenomenon yet produced. These studies show first that the amount of fatigue is greatest at higher frequencies, although this conclusion depends a little on how the data are plotted. When all fatiguing has been done with tones of equal intensity, the resulting fatiguing contours resemble the loudness contours, changing shape with different intensity levels. When all tones have the same loudness level, then fatigue is decidedly greater at the higher frequencies. When, however, all tones have the same sensation level, the effect of frequency on the amount of fatigue is slight.

The effect of the duration of the fatiguing tone on the amount of fatigue is very slight. Between 0.1 and 5 sec., there is no cumulative fatigue. Thus, it seems that an adaptation is quickly set up and that further stimulation has little effect on this adaptation. The recovery from fatigue is very rapid. It is complete within 0.3 sec. after stimulation with tones of 70 db. sensation level. Recovery is complete in correspondingly shorter times at lower intensities, and the recovery rate is approximately 200 db. per sec.

Measurement of Neural Fatigue

There have been a few studies on auditory fatigue in the neural system, and for the most part, these studies have made use of the neural action potential as recorded at the round window. Because of the clearly defined response to clicks and because clicks represent wide-band stimulation, they have been the stimulus most frequently employed.

In two very similar studies by Hawkins & Kniazuk (37) and by Rosenblith, Galambos & Hirsh (68), the effect of loud tones and noise on the action

potential was measured. These measures show somewhat the same picture as is shown in human studies, with the exception that the time scales for recovery of function tend to differ. After stimulation by noise or tones, the neural response to clicks is reduced and gradually comes back to its former amplitude. Under some conditions it shows supernormal recovery, with a larger amplitude of response than was indicated before stimulation. Recovery of response at the round window is closely paralleled by recovery of response in the cortex.

Again paralleling work on humans, the greatest loss of response occurs with prestimulation by those frequencies to which the ear is most sensitive [Rosenblith (67)]. The fatigue is greatest for weak clicks and least for higher intensity clicks. This effect is equivalent to the recruitment effect in temporary deafness. A similar picture is obtained when the click response is masked simultaneously by either noise or pure tones. In fact, the similarity between these studies on the masking of clicks [including those on humans by Hirsh, Rosenblith & Ward (41)] makes the distinction between short duration auditory fatigue and the longer duration fatigue resulting from more intense stimulation much more obscure. It is possible that both effects are the same, of course, but more work on the nature of the similarities and differences will be necessary before a clear picture is obtained.

Loudness Recruitment

The term "loudness recruitment" is used to refer to the fact that under certain conditions loudness increases abnormally fast as intensity increases. For example, a tone which is masked by noise may have a threshold as much as 40 or 50 db. higher than the threshold for that same tone in the quiet. Yet when both tones are raised the same number of decibels above the threshold, the masked tone will appear considerably louder than the unmasked tone. Actually, they will appear equally loud if both tones are at the same intensity, indicating that the original masking has little effect on the loudness of the tones at suprathreshold levels.

A similar effect occurs with perceptual deafness. A person with perceptual deafness may have a threshold which is considerably elevated with respect to normal but hear tones with the same loudness when the tones are sufficiently high above the threshold. In cases of conductive deafness, this phenomenon does not occur. Because of this difference between the two major categories of deafness, the presence or absence of loudness recruitment has become one of the major diagnostic criteria for type of deafness.

The only certain way of determining the presence of recruitment is to compare the growth in loudness in a normal ear with that in a deaf ear (or masked ear). Most deaf people, however, are deaf in both ears, and this produces a difficulty in determining the presence of recruitment: the deaf person has no standard of comparison. This state of affairs has led to many attempts to find ways of determining the presence of recruitment monaurally. All of the suggested techniques have one thing in common: they are tech-

XUM

niques which measure some psychophysical phenomenon which is itself a function of loudness rather than sensation level. Better still, of course, are those phenomena which, in the normal ear, show distinctly different relationships to loudness and to sensation level. One of the best of such phenomena has been the difference limen measurement, which is normally high at low loudness levels, but gets smaller and smaller at higher loudnesses.

Békésy (3) suggested the use of the absolute threshold variability as a good measure of recruitment, basing his suggestion on the argument that the range of threshold intensities should be smaller if recruitment exists, because a given range of intensities would represent a much greater range of loudness. Lüscher & Zwislocki (56) suggest that the intensive difference limen (DL) at a sensation level of 40 db. is a good measure, because the effective loudness for the recruiting and the nonrecruiting ear are quite different at that level.

Lidén & Nilsson (55) and Denes & Naunton (16) discuss the advantages and disadvantages of several methods of determining the existence of recruitment. In general, they feel that Békésy's method is quite reliable, but feel that methods which require a single DL determination at higher sensation levels are too subject to error due to large individual differences. To offset this disadvantage, Denes & Naunton suggest measuring the DL at 4 and at 44 db. above the threshold and taking the difference between these two DL's as the measure of recruitment. If no recruitment is present, the relative DL will decrease considerably with the increase in intensity. If recruitment exists, however, there will be little difference in the relative DL's. They are suggesting, in a sense, that each listener serve as his own control to determine the size of the DL.

All of these methods assume, of course, that the DL is a function of loudness. There is little doubt that the DL does change with loudness, although experiments by Harris (34) on the methodology for determining the DL suggest that the success of the DL methods in measuring recruitment may well depend on which method is used. When he used a forced-choice method of constants, he found very little change in the DL as intensity was decreased. He feels that this method is a real measure of sensitivity, while other methods are too much a measure of apprehension of the observer. At any rate, as long as some methods show a difference in the DL as a function of intensity, these techniques can be used to determine the presence of recruitment.

Two other methods of determining the presence of recruitment have also been proposed. Both of these methods make use of the fact that in perceptively deaf persons, the range of intensities between threshold and pain is quite small compared to the normal. Zangemeister (86) suggests using the range between threshold and pain for speech stimulation as a measure of recruitment, while Laskiewicz (50) prefers the cochleociliary reflex. In clinical practice, both techniques seem to offer some success.

The relations mentioned above do not, of course, explain the nature of

recruitment. Hallpike & Hood (32), in a continuation of their studies on auditory adaptation, point out that two effects occur with recruitment which are the same as those occurring after auditory adaptation. After adaptation, when a tone first comes on, it has normal loudness. However, the loudness decreases very rapidly after that. These same two effects occur with cases showing recruitment, and the authors suspect that the type of deafness which shows recruitment is very similar to the type of deafness which occurs after prolonged stimulation to loud sounds. In the previous discussion on experimental deafness, it was pointed out that such deafness always shows the recruitment effect. Thus, the argument of these authors that the two are similar is probably well taken.

Pitch

Several studies have been reported on the pattern aspect of tones. Heise & Miller (38) determined the pitch threshold for inclusion of tones in different pitch patterns. The pitch patterns used were ascending pitches, descending pitches, descending then ascending pitches, and ascending then descending pitches. The tone to be integrated into the pattern was always the middle tone in the sequence. They found that the thresholds are determined by the entire pattern. Thus, later tones have as much influence as earlier tones.

In another study, Miller & Heise (58) determined the "trill" threshold. When two tones of different frequency are alternated (five times per second in their experiment), the tones are heard as a continuous up and down when the frequency difference is small. When the frequency difference is large, however, the tones are heard as two unrelated sets of interrupted tones. The maximum frequency difference which allowed the pattern to be heard as a "trill" was determined as a function of frequency. It was found to increase continuously with frequency and was not simply related to the DL for frequency.

In a similar kind of study by Corso & Lewis (10), listeners were asked to state preferences for different amounts and rates of frequency vibrato in complex tones. The preferred conditions were at about 6.5 modulations per second, with a range of 0.25 of a whole musical step. Trained musicians agree generally with untrained listeners, but preferred a narrower range of modulation.

Taubman (74) found that the accuracy with which observers could count successive short tones depended both on the rate and the number of tones. At rates of eight tones per second, perfect accuracy was possible up to 10 successive tones; but at rates of 10 tones per second, perfect accuracy was possible only up to four successive tones.

The ability to hear binaural beats depends both on the frequency and the frequency difference between the tones at the two ears, according to experiments reported by Licklider, Webster & Hedlun (54). At about 400 c.p.s., the frequency difference which will allow the hearing of beats is

largest, and the difference decreases for both higher and lower frequencies. This interaction effect in part explains the widely divergent reports of the upper frequency limits for binaural beats.

One more paper on pitch, presented by Deutsch (17), shows a binaural aftereffect. If one ear is stimulated continuously by a pure tone, the DL for pitch at the opposite ear is decreased. He explains this result in terms of a Köhler-Wallach type of aftereffect. However, the data are slim and show enough variability that it would be better to wait for additional confirmation before taking this result at its face value.

Localization of Sound

The only psychophysical research on sound localization has not been concerned with interaural effects. Arnoult (2), confirming the previous work of Clark & Graybiel (9), showed that after an observer has been rotated, sounds are located in the direction of the original displacement, i.e., in a direction opposite to the postrotational movement.

Cotzin & Dallenbach (11) report a series of experiments on the ability of blind persons to locate objects with sound cues. When thermal noise and various pure tones were used, the listeners had to determine the location of an end wall by using only the sound cues picked up from a microphone placed below the movable source of sound. Accuracy was quite good with thermal noise and with pure tones at high frequencies. From this they conclude that the primary cue is the Doppler shift at the higher frequencies.

Binaural Interaction in the Nervous System

Although relatively little work has been done in the past year on the psychophysics of binaural interaction, some excellent progress has been made on understanding the possible mechanisms for interaural effects in the nervous system. In experiments reported by Rosenzweig & Rosenblith (69), the neural action potential recorded at the round window is suppressed when the click producing the potential is preceded by another click. Similar relations exist at the cortex. When one click stimulates one ear, however, and the second click the other ear, there is summation of the two potentials at the cortex if the two clicks occur close enough together in time. The amplitude of the summated response at the cortex depends on the time relations at the two ears in respect to the two sides of the cortex. A left-right order produces a larger response in the right cortex, and vice versa. Thus, we have the interaural time relations showing up as amplitude differences at the two sides of the cortex.

Ades & Brookhart (1) measured amplitude and latency of responses to clicks at various points in the nervous system and found that both the amplitude and duration of the response increase at higher neural centers. Furthermore, at the inferior colliculus, a contralateral stimulus gives a shorter latency than an ipsilateral stimulus, by an amount indicating one

less synapse. Thus, here is clear evidence of an interaction effect at the level of the colliculus.

Galambos, Rosenblith & Rosenzweig (30) show neural interaction at an even lower level, in fact at the level of the cochlea itself. When one ear is stimulated by a click which precedes a click presented to the other ear, the neural response recorded at the round window of the second ear is modified. This modification is destroyed when the first ear is destroyed. Thus, there seems to be cross-connections of some kind at the level of the cochlea, and the interaction effects are at least partly set up at that point in the auditory system.

Hearing of Speech

In order to discover how speech is understood, Licklider and his coworkers have used various types of amplitude distortion to determine their effects on the intelligibility of speech. The extreme of one form of amplitude distortion (peak clipping) is to produce a square pulse every time the speech wave form crosses the time axis. That is to say, the speech wave is allowed to have only two intensities. In a recent paper, Licklider (52) has further quantized the speech wave form by allowing the wave to change between these two amplitudes only at certain predetermined time intervals; thus, the speech is quantized both in time and amplitude. With such a speech wave form, it is possible to measure intelligibility as a function of the number of quanta per second, and he finds that between 8 and 10 thousand quanta per second are required for intelligible speech.

Kock (48), in an extension of work of the type previously reported by Licklider (51) and by Hirsh (39), has shown that speech intelligibility is best when the speech and noise arrive at the two ears from two different directions. In review of this type of work, Hirsh (40) has pointed out the implications of these findings in the design of binaural hearing aids. Only with a binaural hearing aid can the listener take advantage of the different directions of the speech and the interfering noise.

In experiments using another type of distortion, Flanagan (23) divided the speech spectrum into 20 equal articulation bands and advanced or delayed in time some of these bands with respect to others. He found the greatest effect on intelligibility when about half the bands were delayed with respect to the other half and when the time discrepancy was about 0.25 sec. The effect is also more marked when the shifted bands are in the center of the spectrum.

Frequency Selectivity in the Nervous System

It has been known for some years, particularly since the work of Woolsey & Walzl (85), that there is some form of frequency localization in the cortex. And, of course, frequency localization at the level of single auditory nerve fibers was clearly established by the work of Galambos & Davis (29). Usually,

however, the localization shown is of a fairly crude sort and in no sense explains the precision of frequency discrimination actually demonstrable in psychophysical experiments.

A new technique has been applied to this problem of frequency localization at the cortex. Tunturi (75) first anesthetized dogs with sodium pentobarbital and then strychninized the exposed cortex with small (1 mm.×1mm.) strips of filter paper. Then the ear was stimulated with various frequencies; strychnine spikes occurred if the proper frequencies were used for a particular area. With this technique, Tunturi found that in the middle ectosylvian area fibers for each frequency terminate in a band no wider than 0.2 mm. and about 5 to 7 mm. in length, extending transversely across the gyrus. These bands are arranged as a series of parallel strips with a width of 2 mm. per octave betwen 250 and 8,000 c.p.s., the higher frequencies being anterior. He obtained response curves for each area and could discriminate between 0.1 octave strips.

In many ways, these results are a little surprising because the precision of frequency localization is so much greater than shown previously. It is possible that such precision exists in dogs, but not in cats or other animals. It does not seem likely, however, that the difference in animals could account for such a marked difference in precision of localization. It seems likely that the difference in technique has allowed the precision to show up. This research is important, and it is to be hoped that it will soon be verified in other laboratories. It is also to be hoped that an explanation of the real cause of the differences will soon be available.

In another type of evidence of frequency selectivity of the nervous system, Galambos (28) has shown that the neural response to a click at the medial geniculates can be suppressed by a pure tone. This suppression, or inhibition, is frequency selective. Thus, at a particular point in the medial geniculate, only a narrow band of frequencies will suppress the click-response. As an example, in one case only frequencies in a 100 c.p.s. band around 2,400 c.p.s. were able to suppress the response.

Auditory Theory

Since the time of Békésy's already classic measurements of the displacement patterns on the basilar membrane (4, 5), theorizing about the nature of hearing has become somewhat more restricted. There now is no question that a form of frequency analysis occurs on the basilar membrane, nor is there any question that the analysis is not the kind that would be performed by simple resonating elements. However, theorizing of a kind does continue, and it continues to be concerned primarily with the nature of pitch discrimination.

Both Ranke (66) and Zwislocki (87) have proposed mechanisms to explain the vibration patterns measured by Békésy. These two theories are similar in that they both attempt to explain the vibration patterns on the basis of the hydrodynamic properties of the cochlea. They differ, however,

XUM

in that Ranke makes the role of relative velocities very important, while Zwislocki claims that the velocity aspect is not important. Rather, the maximum is due to the elasticity and damping properties of the cochlea. At this point there seems to be little to allow a choice between the two explanations insofar as they can predict the sort of patterns that Békésy has actually measured. In terms of the ability to predict the order of discrimination precision which psychophysical results show, however, Ranke's emphasis of the velocity aspect of the wave propagation has some real advantages.

There is still quite a discrepancy between the broad patterns of vibration on the basilar membrane and the precise frequency discrimination which we know is possible. Wever (80) points to this difficulty in continuing his argument that pitch is mediated by the frequency of neural discharge, particularly at the lower frequencies. However, Huggins & Licklider (43) have pointed out several ways in which the broad pattern of excitation in the cochlea can be transformed into much sharper patterns. They class the various possibilities into those which are primarily mechanical and those which are primarily neural. Most of these possibilities are based on the fact that some derivative of the displacement function can have much sharper peaks than the displacement function itself, and the possibilities are in terms of which derivative function acts as the actual determiner of the pattern of stimulation. The four mechanical possibilities are as follows:

(*a*) *The eddy hypothesis.*—Békésy (5) has shown that eddy currents are set up in the cochlea and that these currents produce a unidirectional pressure which is much more restricted in breadth than the alternating pressure. This hypothesis is one that does not involve the assumption of a derivative function. The eddies can be considered, however, as partial rectifications.

(*b*) *The gradient hypothesis.*—This hypothesis assumes that the neural response is not to the displacement function, but rather to its first derivative, the gradient. The gradient pattern is much sharper than the displacement pattern.

(*c*) *The curvature hypothesis.*—This hypothesis assumes that the neural response is to the second derivative or the curvature. The curvature pattern is even sharper than the gradient pattern.

(*d*) *The beam hypothesis.*—While both of the preceding two hypotheses provide sharper patterns, the actual force acting on the hair cells would be very small. One possibility is that the tectorial membrane acts like a bridge rigidly coupled to the hair cells and as such can be a force amplifier. The action of this bridge would be the fourth derivative of the original displacement function.

The authors suggest three different neural hypotheses. One of these assumes that there are neural fibers which act as piezoelectric transducers, setting up high electric potentials which are actually the sixth derivative of the displacement function. The other two suggested hypotheses are basically similar in that they require the nervous system to perform a double differentiation. These possibilities are more real than they seem at first glance be-

cause an approximate differentiation can be performed with a simple combination of facilitating and inhibiting fibers. The nature of inhibiting fibers would be the first serious question, but there is little doubt that such fibers exist.

Although all of these possibilities contain some merit and some real plausibility, none of them has the simplicity of Ranke's proposal (66). The authors have classed Ranke's hypothesis as a mechanical action mechanism, but it seems to the reviewer that it is actually a neural mechanism. Basically, Ranke's hypothesis is this: as the acoustic wave travels down the cochlea, its velocity is different at different lengths. The velocity will be slowest at the point of maximum amplitude of vibration and will be faster both before and after that point. If we make the quite safe assumption that neurons will fire only when there is summation of local excitations, then firing will occur in only a narrow region, i.e., the region where the velocity of the wave is slow enough to allow successive excitations to summate. This same argument can be used, of course, at synaptic levels higher in the nervous system without loss of plausibility. In fact, our present knowledge of synaptic action seems to the reviewer to make this hypothesis highly plausible.

Duplex theory of pitch.—In spite of the fact that localization has been obtained for low frequencies on the basilar membrane and in the cortex, there are still occasional attempts to explain the mediation of pitch as being due to the periodicity of the wave form. Licklider (53) proposes a duplex theory of pitch in which a place analysis operates for the higher frequencies and a time analysis operates for the lower frequencies. With his theory, the initial time analysis is changed by an autocorrelational process in the nervous system to a place analysis at higher centers of the nervous system. Thus, he eventually gets all frequencies to be discriminated by place, although he starts out with some of them being discriminated by time. Licklider proposes this theory to explain certain facts which seem to him to be at variance with a simple place theory of pitch. To the reviewer, however, it seems that the facts he is attempting to explain are incorrectly interpreted and that, when they are correctly interpreted, he has an argument for which there are no facts that need to be explained. Such duplex mechanisms as those proposed by Licklider and by Wever (81) violate the law of parsimony to an extent completely unjustified by the facts.

In an experiment by Miller & Taylor (60), observers were asked to match a sine wave to the rate of interruption of white noise. Since the interrupted noise had no frequency component corresponding to the rate of interruption, if observers could match the sine wave to the rate of interruption, presumably pitch was contained in the interrupted noise. The only possible explanation, then, would be that pitch is mediated by the time pattern. However, observers were able to match the two frequencies with only very poor success. Their best three observers were able to match the sine wave to the

XUM

interrupted noise as accurately as to square waves and pulses up to 60 interruptions per second. At higher frequencies, the accuracy of matches to the interrupted noise fell off very rapidly. Yet intermittency in the noise could be experienced at interruption rates of over 1,000 interruptions per second. If the periodicity can produce an experience of pitch, why should not observers be able to match sine waves and interrupted noise at much higher frequencies? We have here clear evidence that intermittency is possible without pitch.

The basic difficulty seems to be that the range of frequencies over which we can experience intermittency overlaps the range of frequencies over which we experience pitch. We are then led to feel that the experience should be the same for intermittency and for the pitch of pure tones. In other modalities, however, intermittency can be experienced in this same range of frequencies. In vision, intermittency can be experienced up to 60 or more interruptions per second, and in this modality, the limiting factor is undoubtedly the chemical processes of the eye, not the capacity of the nervous system. Why should it be surprising that we can experience intermittency in the same range of frequencies in audition?

To the reviewer, the explanation of these data is that up to 60 c.p.s., observers were able to match the intermittency of the noise to the intermittency of the sine wave. At higher rates of noise interruption, the intermittency of the noise is still audible, but the pitch of the sine wave has become dominant; no match is possible because the experience of pitch and the experience of intermittency are simply two different things. It is unfortunate that the frequency ranges for the two overlap in hearing, but reference to our ability to experience intermittency in other modalities should remind us that intermittency can be experienced and that it does not have to be called pitch.

Davis, Silverman & McAuliffe (15) have also reported data which they feel need a duplex theory. They stimulated the ear with pulses which were filtered so that none of the fundamental frequency (the pulsing rate) was allowed to come through. Because observers were able to hear both the pitch corresponding to the filter frequency and the buzz corresponding to the repetition rate, the authors argue that two mechanisms are operating and that both mechanisms are pitch producing.

The same rationale concerning the difference between intermittency and pitch applies in this case. In addition, this type of test is a very poor one because of the pattern of frequency components which are produced by such a stimulus. When a repeated pulse is put through a filter, what remains is a series of frequency components whose spacings are the same as the frequency of the repetition rate. For example, if a pulse with a repetition rate of 100 pulses per second is put through a filter which rejects all frequencies below 500 c.p.s., the remaining frequencies will be spaced 100 c.p.s. apart, i.e., at 600, 700, 800 c.p.s., etc., and all difference tones created in the ear will have a frequency of 100 c.p.s.—the same as that of the repetition rate. In addition,

XUM

simple principles of Gestalt psychology would suggest that with such a pattern of stimulation on the basilar membrane, there would be a strong tendency to complete it by filling in the "missing fundamental."

Thus, such tests as these cannot be considered crucial. The actual data from experiments such as those reported by Miller & Taylor show that intermittency can be experienced under conditions in which the intermittency has no definable pitch. It seems to the reviewer that duplex theories of pitch perception are at the present aiming only at straw men.

LITERATURE CITED

1. Ades, H. W., and Brookhart, J. M., *J. Neurophysiol.*, **13,** 189–205 (1950)
2. Arnoult, M. D., *Am. J. Psychol.*, **63,** 229–36 (1950)
3. Békésy, G. von, *Acta Oto-Laryngol.*, **35,** 411–22 (1947)
4. Békésy, G. von, *J. Acoust. Soc. Am.*, **19,** 452–60 (1947)
5. Békésy, G. von, *J. Acoust. Soc. Am.*, **21,** 245–54 (1949)
6. Békésy, G. von, *J. Acoust. Soc. Am.*, **23,** 18–28 (1951)
7. Békésy, G. von, *J. Acoust. Soc. Am.*, **23,** 29–35 (1951)
8. Beranek, L. L., Marshall, J. L., Cudworth, A. L., and Peterson, A. P. G., *J. Acoust. Soc. Am.*, **23,** 261–69 (1951)
9. Clark, B., and Graybiel, A., *J. Psychol.*, **28,** 235–44 (1949)
10. Corso, J. F., and Lewis, D., *J. Applied Psychol.*, **34,** 206–12 (1950)
11. Cotzin, M., and Dallenbach, K. M., *Am. J. Psychol.*, **63,** 485–515 (1950)
12. Davis, H., Fernández, C., and McAuliffe, D. R., *Proc. Natl. Acad. Sci.*, **36,** 580–87 (1950)
13. Davis, H., Gernandt, B. E., and Riesco-MacClure, J. S., *J. Neurophysiol.*, **13,** 73–87 (1950)
14. Davis, H., Morgan, C. T., Hawkins, J. E., Jr., Galambos, R., and Smith, F. W., *Acta Oto-Laryngol.*, Suppl. 88, 1–56 (1950)
15. Davis, H., Silverman, S. R., and McAuliffe, D. R., *J. Acoust. Soc. Am.*, **23,** 40–42 (1951)
16. Denes, P., and Naunton, R. F., *J. Laryngol. Otol.*, **64,** 375–98 (1950)
17. Deutsch, J. A., *Quart. J. Exptl. Psychol.*, **3,** 43–46 (1951)
18. Egan, J. P., and Hake, H. W., *J. Acoust. Soc. Am.*, **22,** 622–30 (1950)
19. Egan, J. P., and Klumpp, R. G., *J. Acoust. Soc. Am.*, **23,** 275–86 (1951)
20. Egan, J. P., and Meyer, D. R., *J. Acoust. Soc. Am.*, **22,** 827–33 (1950)
21. Eisenberg, P., and Chinn, H. A., *J. Exptl. Psychol.*, **35,** 374–92 (1945)
22. Fernández, C., Gernandt, B. E., Davis, H., and McAuliffe, D. R., *Proc. Soc. Exptl. Biol. Med.*, **75,** 452–55 (1950)
23. Flanagan, J. L., *J. Acoust. Soc. Am.*, **23,** 303–7 (1951)
24. Fletcher, H., *Revs. Modern Phys.*, **12,** 47–65 (1940)
25. Fletcher, H., *Acta Oto-Laryngol.*, Suppl. 90, 26–37 (1950)
26. Fletcher, H., and Munson, W. A., *J. Acoust. Soc. Am.*, **5,** 82–108 (1933)
27. Fletcher, H., and Munson, W. A., *J. Acoust. Soc. Am.*, **9,** 1–10 (1937)
28. Galambos, R., *J. Acoust. Soc. Am.*, **22,** 785–91 (1950)
29. Galambos, R., and Davis, H., *J. Neurophysiol.*, **6,** 39–57 (1943)
30. Galambos, R., Rosenblith, W. A., and Rosenzweig, M. R., *Experentia*, **6,** 438–46 (1950)
31. Gardner, M. B., *J. Acoust. Soc. Am.*, **19,** 178–90 (1947)
32. Hallpike, C. S., and Hood, J. D., *J. Acoust. Soc. Am.*, **23,** 270–74 (1951)

XUM

33. Hardy, W. G., and Bordley, J. E., *J. Speech Hearing Disorders* (In press)
34. Harris, J. D., *Am. J. Psychol.*, **63,** 409–21 (1950)
35. Harris, J. D., *Rept. No. 168*, **9,** 291–300 (Naval Med. Research Lab., U. S. Submarine Base, New London, Conn., 1950)
36. Harris, J. D., Rawnsley, A. I., and Kelsey, P. A., *Rept. No. 167*, **9,** 278–90 (Naval Med. Research Lab., U. S. Submarine Base, New London, Conn., 1950)
37. Hawkins, J. E., Jr., and Kniazuk, M., *Science*, **111,** 567–68 (1950)
38. Heise, G. A., and Miller, G. A., *Am. J. Psychol.*, **64,** 68–77 (1951)
39. Hirsh, I. J., *J. Acoust. Soc. Am.*, **22,** 196–200 (1950)
40. Hirsh, I. J., *J. Speech Hearing Disorders*, **15,** 114–23 (1950)
41. Hirsh, I. J., Rosenblith, W. A., and Ward, W. D., *J. Acoust. Soc. Am.*, **22,** 631–37 (1950)
42. Hood, J. D., *Acta Oto-Laryngol.*, Suppl. 92, **1–57** (1950)
43. Huggins, W. H., and Licklider, J. C. R., *J. Acoust. Soc. Am.*, **23,** 290–99 (1951)
44. Jacobson, H., *Science*, **112,** 143–44 (1950)
45. Kahana, L., Rosenblith, W. A., and Galambos, R., *Am. J. Physiol.*, **163,** 213–23 (1950)
46. Klumpp, R. G., and Egan, J. P., *J. Acoust. Soc. Am.*, **23,** 113–14 (1951)
47. Knapp, P. H., and Gold, B. H., *Psychosomat. Med.*, **12,** 6–22 (1950)
48. Kock, W. E., *J. Acoust. Soc. Am.*, **22,** 801–4 (1950)
49. Kryter, K. D., *J. Speech Hearing Disorders*, Monogr. Suppl. 1, 1–95 (1950)
50. Laskiewicz, A., *Acta Oto-Laryngol.*, **38,** 403–16 (1950)
51. Licklider, J. C. R., *J. Acoust. Soc. Am.*, **20,** 150–59 (1948)
52. Licklider, J. C. R., *J. Acoust. Soc. Am.*, **22,** 820–23 (1950)
53. Licklider, J. C. R., *J. Acoust. Soc. Am.* (In press)
54. Licklider, J. C. R., Webster, J. C., and Hedlun, J. M., *J. Acoust. Soc. Am.*, **22,** 468–73 (1950)
55. Lidén, G., and Nilsson, G., *Acta Oto-Laryngol.*, **38,** 521–27 (1950)
56. Lüscher, E., and Zwislocki, J., *Arch. Ohren-, Nasen- Kehlkopfheilk.*, **155,** 323–34 (1948)
57. Maré, G. de, and Roesler, G., *Acta Oto-Laryngol.*, **38,** 179–90 (1950)
58. Miller, G. A., and Heise, G. A., *J. Acoust. Soc. Am.*, **22,** 637–38 (1950)
59. Miller, G. A., Rosenblith, W. A., Galambos, R., and Hirsh, I. J., *A Bibliography in Audition* (Harvard Univ. Press, Cambridge, Mass., 2 Vols., 1950)
60. Miller, G. A., and Taylor, W. G., *J. Acoust. Soc. Am.*, **20,** 171–82 (1948)
61. Munson, W. A., and Gardner, M. B., *J. Acoust. Soc. Am.*, **22,** 177–90 (1950)
62. Munson, W. A., and Wiener, F. M., *J. Acoust. Soc. Am.*, **22,** 382–86 (1950)
63. Myers, C. K., and Harris, J. D., *Rept. No. 165*, **9,** 230–57 (Naval Med. Research Lab., U. S. Submarine Base, New London, Conn., 1950)
64. Ogden, F. W., *Laryngoscope*, **60,** 993–1012 (1950)
65. Olson, H. F., *J. Acoust. Soc. Am.*, **19,** 549–55 (1947)
66. Ranke, O. F., *J. Acoust. Soc. Am.*, **22,** 772–77 (1950)
67. Rosenblith, W. A., *J. Acoust. Soc. Am.*, **22,** 792–800 (1950)
68. Rosenblith, W. A., Galambos, R., and Hirsh, I. J., *Science*, **111,** 569–71 (1950)
69. Rosenzweig, M. R., and Rosenblith, W. A., *J. Acoust. Soc. Am.*, **22,** 878–80 (1950)
70. Schafer, T. H., *J. Exptl. Psychol.*, **40,** 365–71 (1950)
71. Schafer, T. H., Gales, R. S., Shewmaker, C. A., and Thompson, P. O., *J. Acoust. Soc. Am.*, **22,** 490–96 (1950)

72. Steinberg, J. C., Montgomery, H. C., and Gardner, M. B., *J. Acoust. Soc. Am.*, **12,** 291–301 (1940)
73. Stuntz, S. E., *Rept. No. 163*, **9,** 184–94 (Naval Med. Research Lab., U. S. Submarine Base, New London, Conn., 1950)
74. Taubman, R. E., *J. Gen. Psychol.*, **43,** 167–94 (1950)
75. Tunturi, A. R., *Am. J. Physiol.*, **162,** 489–502 (1950)
76. Warren, J. M., and Egan, J. P., *J. Acoust. Soc. Am.*, **23,** 111–13 (1951)
77. Webster, J. C., Himes, H. W., and Lichtenstein, M., *J. Acoust. Soc. Am.*, **22,** 473–83 (1950)
78. Webster, J. C., Lichtenstein, M., and Gales, R. S., *J. Acoust. Soc. Am.*, **22,** 483–90 (1950)
79. Wegel, R. L., and Lane, C. E., *Phys. Rev.*, **23,** 266–85 (1924)
80. Wever, E. G., *J. Acoust. Soc. Am.*, **23,** 287–89 (1951)
81. Wever, E. G., *Theory of Hearing* (John Wiley & Sons, Inc., New York, 484 pp., 1949)
82. Wheeler, D. E., *Arch. Otolaryngol.*, **51,** 344–55 (1950)
83. Wilson, W. H., *Ann. Otol. Rhinol. Laryngol.*, **59,** 399–405 (1950)
84. Wing, K. G., *Rept. No. 164*, **9,** 195–229 (Naval Med. Research Lab., U. S. Submarine Base, New London, Conn., 1950)
85. Woolsey, C. N., and Walzl, E. M., *Bull. Johns Hopkins Hosp.*, **71,** 315–44 (1942)
86. Zangemeister, H. E., *Acta Oto-Laryngol.*, **38,** 484–97 (1950)
87. Zwislocki, J., *J. Acoust. Soc. Am.*, **22,** 778–84 (1950)

SOMESTHESIS AND THE CHEMICAL SENSES[1,2]

By G. R. Wendt

Department of Psychology, University of Rochester, Rochester, New York

The research published this year shows considerable interest in the peripheral mechanisms of pain, pain scaling, effects of frontal lobotomy on pain, action of the muscle spindle, physiology and physics of the semicircular canals and of the otolith organs, effects of various drugs (especially the motion sickness preventives) on the vestibular system, and theory of the mechanisms of stimulation of smell and taste.

PAIN[3]

Edwards (44), citing 100 publications, has reviewed recent research on four related problems of pain: definition of pain, psychophysics of pain, peripheral nervous mechanisms involved in pain perception, and role of the central nervous system in certain abnormalities of pain perception.

Peripheral mechanisms.—The eighth of a series of studies supporting the theory of histamine or a histamine-like substance as the chemical mediator of cutaneous pain was reported by Rosenthal (141). Forty subjects received injections of various dilute solutions of histamine into the superficial layers of the skin. These injections yielded both an immediate and a delayed pain response. The former varied from a pricking, tingling sensation to acute pain, depending upon the concentration of drug. As little as 54 molecules of histamine were needed to produce pain. The delayed effect, appearing after a mean of 10 sec. with higher concentrations, was variouly described as pricking, stinging, tingling, burning, dull, and sharp pain. Sensations of cold, warmth, and pressure were occasionally reported.

Hardy, Wolff & Goodell (77) made an extensive study (23 subjects and 500 series) of natural and of experimental hyperalgesia of the skin. The latter was produced by ultraviolet light, thermal radiation, faradic stimulation, and still other means; the former was the hyperalgesia resulting from injured skin. Natural hyperalgesia showed lowered thresholds to mechanical and thermal stimuli (on the dol scale) and also intensification and increased duration of pain. In experimental hyperalgesia, there was more intense and prolonged pain, but no change in threshold. There was a delay of 2 to 10 min. in its development.

Cutaneous itch and tickle were analyzed by Graham, Goodell & Wolff

[1] The survey of the literature to which this review pertains was completed about June, 1951.

[2] The preparation of this chapter was made possible by use of funds from a research contract between the Office of Naval Research and the University of Rochester.

[3] Paul Swartz, Graduate Research Assistant, aided with the bibliographic work for this section.

(66). They found that these were wholly due to weak pain stimulation and that no other receptors were involved. When itching was present, the pain threshold was lowered in that area of skin, but tickle and itch did not appear in hyperalgesic areas. Tickle and itch could be abolished by a pin prick in neighboring skin. Two different itches, one pricking and the other burning, were attributed to the two types of pain fibers from the skin. The authors suggested that the sensation of itch results from the presence in the spinal cord of impulses traveling in circular circuits. These, it is thought, are broken up by strong stimulation.

In three humans, Ebaugh, Bird & Hardy (42) found that the sternal bone marrow, injected with hot or cold water (1 ml.) yielded no temperature sensations, but did give a burning and aching pain at the beginning of each injection.

Central mechanisms.—Kennard (100) produced cutaneous hyperalgesia in the cat by applying alumina cream to the dorsal aspect of the lumbar spinal cord. States of hyperesthesia and hyperalgesia developed over a period of days to weeks and lasted for at least nine months (the longest period of her observations). Light touch, heavy touch, and pain stimuli all became apparently painful to the animal. Light touch was the most disturbing type of stimulus. Kennard compared the state to the causalgias found in man.

The spinal levels of entry of nerves mediating pain from the gonads have been investigated by Bors (24). In 53 human cases of spinal cord injury, he gave stimuli sufficient to cause gonadal pain. The tenth thoracic segment for sensation of pinprick was the upper level for the experiencing of gonadal pain, but the intensity of the pain appeared to increase as the level for pinprick descended below the tenth thoracic to the upper lumbar segments.

In a recent book, Penfield & Rasmussen (134) summarized conclusions reached over a period of years from studies of electrical stimulation and of surgical excisions of the human cerebral cortex. They reaffirm their conviction that pain has no cortical representation. However, in view of the paucity of sensory qualities resulting from such stimulation, it seems to the reviewer that this conclusion is not a necessary one.

Effects of prefrontal lobotomy.—Smolik (151) reported favorable results from bilateral prefrontal lobotomy for relief of discomfort from intractable pain. Le Beau (109) reported similar results from bilateral topectomy of cortical areas 9, 10, and 46, and Rowe & Moyer (142) also reported that 8 of 16 cases were relieved by unilateral lobotomy. In three cases of failure, Rowe & Moyer found that a subsequent operation on the other side was successful. King, Clausen & Scarff (101) applied radiant heat by means of the Hardy-Wolff-Goodell apparatus to determine pain thresholds before and after unilateral lobotomy in six patients suffering from intractable pain. Five of these were improved by operation. The one patient unrelieved by the operation showed elevated thresholds after operation. In 1948, Chapman, Rose & Solomon reported a similar threshold study of motor withdrawal

from radiant heat in prefrontal lobotomy patients operated for relief of psychotic symptoms in which they found decreased thresholds. They have recently reported (32) a follow-up study of 13 of these patients after 13 to 24 months. The decreased tolerance for pain was no longer present at the time of this second test. Krayenbuhl (105) related relief of pain to preoperative personality.

Effects of drugs.—In two studies use of the Hardy-Wolff-Goodell apparatus to repeat earlier work of Wolff *et al.* on the effects of analgesic drugs on pain thresholds has been reported. Kuhn & Bromiley (106) compared sucrose, 0.9 gm. of aspirin, and 0.09 g., of phenobarbital and found negligible analgesic effects. Morphine (0.016 gm.) elevated the threshold in some subjects. The authors are adversely critical of the methods used in the earlier studies. Birren, Schapiro & Miller (22) compared aspirin and placebo and found no change in threshold following administration of the drug. Schwartz, Sata & Laszlo (148) used peripheral vasoconstriction in response to painful stimuli as an indicator of the analgesic effects of drugs. Morphine, codeine, and demerol injected subcutaneously differentially elevated the threshold of response, while placebos did not.

Quantification of pain.—Three studies cast doubt on the adequacy of the methods used by the originators of the Hardy-Wolff-Goodell technique. Two of these, Kuhn & Bromiley (106) and Birren, Schapiro & Miller (22), were cited above. Clausen & King (33) also studied the reliability of the method in 31 untrained subjects. In the correlation of first and second threshold determinations, on the forehead r was $+.90$; on the forearm r was $+.73$; between average forehead and average forearm, r was only $+.48$. They found that temporal factors, conducted heat, distractions, reporting method, and area of body stimulated all influenced the thresholds. Other conditions, such as age and educational level of subjects probably also contributed to variance.

Three worthwhile studies have been directed at the experimental study of naturally occurring pain, either by threshold studies or by rough quantification. Keats, Beecher & Mosteller (97) did a carefully designed study of pain in patients during the first 30 hr. followings major surgical operations. The method used rests on the assumption that the magnitude of pain is measured by the amount of morphine (or other experimental drug) necessary to get relief. Morphine and unknown experimental drugs were alternated as the initial drug in successive patients. Although individual variations were slight, they took the form of a normal distribution. The investigators felt that the variation was no greater than is found in experimentally induced pain. They believe that the method is entirely adequate for pharmacological studies and argue for its validity. It seems certain to them that pain, as it is usually produced experimentally, does not reproduce the pain of disease or trauma and that their method avoids any uncertainty about validity. They found that pain-relief categories such, as "none," "slight," "moderate," and "complete relief," were unworkable and that they could use only two categories—"relief" or "no relief."

Gaensler (58) has described a quantitative method for measuring visceral pain thresholds by hydrostatic distention of the biliary duct. In volunteer patinets, during surgical exploration of the common bile duct, he found the threshold of pain to vary from distentions of from 90 mm. to 800 mm. of water. Both the character and the localization or spread of the pain showed some variation. Vignes (159), by use of his utero-tensiometer, related the internal pressure in the uterus during labor contraction to the apparent magnitude of uterine pain. A rise above resting intercontraction level of about 12 mm. Hg was attended by pain, which increased on further rise of pressure. Attempts made to compare the intensity of uterine pain with an induced cutaneous pain were not considered successful because, as the uterine pain rose, the threshold of the skin pain was elevated.

In a study of three paraplegic patients with intractable root pain, Hazouri & Mueller (79) got results similar to those described above. Prior to neural tract section (spinothalamic tractotomy) for relief of pain, all three patients had elevated thresholds both for the perception of and reaction to pain from radiant heat (Hardy-Wolff-Goodell apparatus). After operation the thresholds for perception and for reaction in all three returned to the normal range as determined on a group of 100 paraplegics who did not have pain.

Miscellaneous.—Fedotov (47) reported that painful stress in dogs (strong electric shock for 30 sec. applied to the hips or shoulders) raised the threshold of reflex bending of the paw to touching of the hairs of the paw for several days. He attributed this to activity of the pituitary, induced by stress. Arshavskaia (8) reported that long-continued nociceptive stimulation had no such effects in puppies prior to three or four weeks of age. This he attributed to lack of development of the pain centers of the thalamus and cerebral cortex.

TEMPERATURE SENSATIONS

Jenkins' review (88) of somesthesis in the new *Handbook of Experimental Psychology* contains good sections on cutaneous warm and cold sensations. Hardy (76) also has published a review of recent studies of physiological responses to heat and cold, a small part of which is devoted to temperature sensations.

A major investigation of temperature conduction and thermal adjustment in human skin was done by Hensel (80), who used minute thermoelements placed at different known depths beneath the surface. Both warm objects applied to the skin and changes in air temperature were used as stimuli. The experimental results checked well with theory.

Wright (171) has studied the latency of sensations of warmth as indicated by simple reaction time in 78 human subjects. His stimulus was radiant energy. The minimum reaction time for very intense stimuli was about 1/3 sec., while at threshold the reaction time rose to about 20 sec. There was pronounced spatial summation with stimuli of various extents. By extra-

polation from a plot of reaction time against area, threshold area or threshold intensity could be located. In 22 out of 39 men, the back of the hand was more sensitive than the palm. In another group, for 17 out of 27 subjects the epigastrium was more sensitive than the interscapular region.

Further evidence on the heat exchange necessary to affect the cold and warmth thresholds under various environmental temperatures from 16°C. to 40°C. was offered by Ebaugh & Thauer (43). Warmth thresholds, expressed in millicalories per square centimeter per second did not change within this range (two subjects). Cold thresholds remained relatively constant below 24°C. but almost tripled between 35°C. and 40°C. This was attributed to the effects of vasodilatation.

Bing & Skouby (20) tried to clarify the role of chemical processes in the response of the end organ for cold by changing the chemical environment of the receptors. Various cholinergic and anticholinergic substances were injected near the site of stimulation or transferred by iontophoresis. Response was evaluated by finding the number of cold spots in an area of skin. Acetylcholine produced no statistically significant effect, but the longer-lasting acetyl-β-methylcholine was followed by increases up to 120 per cent of the initial value. The long latency of these effects (5 to 20 min.) is rather puzzling. Prostigmine produced a decrease followed by an increase to 167 per cent of initial values. Atropine, on the other hand, decreased cold spots by about 50 per cent. The authors believed that these effects were not secondary to changes in blood vessels or skin temperature but were direct effects of the substances on the receptors or their associated nerve fibers.

As stated in the preceding section, Ebaugh, Bird & Hardy (42) who injected hot and cold water into the sternal bone marrow of three volunteers got reports of burning and aching pain but no temperature sensations. Summarizing years of study of the human cerebral cortex done at the Montreal Neurological Institute, Penfield & Rasmussen (134) offered the opinion that temperature senses are not, to any important extent, cortically represented.

OTHER CUTANEOUS SENSES

By studies on frog skin, Habgood (74) has contributed to the problem of chemical mediation of skin stimulation by a histamine-like substance. Antidromic stimulation of cutaneous nerves not only caused reddening of the skin (tongue) but also spontaneous discharges during and after stimulation. The results of application of various drugs were consistent with the hypothesis of a histamine-like mediator.

Herman (81) presented detailed case reports of the sex life of four male paraplegic patients with complete cord transection and no remaining sensations from the genitals. Because none of the four reported feeling sexually excited after the injury, he concluded that somesthetic stimuli from the genitals are essential for sexual arousal.

The book by Penfield & Rasmussen (134), in part, concerns topographic localization of the receptors of the body surface on the cerebral cortical sur-

face as revealed by electrical stimulation in conscious human patients and by the effects of cortical excisions. They conclude that the major representation of somatic sensation is in the postcentral gyrus. Although about one-fourth of the stimulated points yielding sensation were precentral, adjacent to similar postcentral points, removal of the precentral areas did not interfere with sensory perception. Ablation of the postcentral gyrus abolished it initially. In general there is a tendency to reproduce on the cortical surface a pattern of representation like the dermatomal pattern on the skin. In a few patients, cortical stimulation just posterior to the motor eye-movement points gave rise to somesthetic sensations referred to the eyes. Abdominal alimentary sensations were occasionally obtained from points, mostly precentral, in the most ventro-lateral region of the sensori-motor strip. It is not certain that these were not sensations aroused by actual motor responses.

Since the discovery by Adrian of a secondary tactile receiving area on the cortex and the amplification of this by Woolsey *et al.*, Penfield & Rasmussen (134) began to look for secondary cortical sensory areas. Sensation was produced in an extremity in eight instances by stimulation at points too far away from the primary area to be part of the ordinary sensory representation of those parts. Like the second sensory cortical area of Adrian, they were separated from the primary sensory area by the face and mouth area. No similar evidence was found for second areas for face, tongue, mouth, or throat.

Anderson & Munson (3) applied alternating currents to the skin of the forearm, using 3/16-in. electrodes. Measurements of difference limen showed that intensity discrimination is not greatly different from that for hearing. Frequency discrimination is as good as hearing in the region up to 30 c.p.s. but becomes progressively poorer. at higher frequencies. Practice was necessary to distinguish 1,000 from 5,000 c.p.s.

Jackson (85) presented case reports showing contralateral reduction of vibratory sensitivity following anterolateral cordotomy at a level corresponding approximately to that of pain and temperature loss. Other evidence led him to conclude that the vibration sense is not mediated by special receptors.

MUSCLE RECEPTORS

Peripheral mechanisms.—Several studies are helping to clarify the action and function of receptors in muscle spindles and in tendinous regions of muscle. Hunt & Kuffler (83, 84) have recorded from cat nerve the discharge from stretch afferents (over 700 units) of various hindlimb muscles. Following Matthews, they identified receptor types A and B. The A units have a low threshold to stretch and cease to discharge during contraction, particularly when shortening is allowed. Under strong contraction, especially at high initial tension, the A units may show discharge, however. It was hypothesized that these units are located in the muscle spindle. The B type, hypothesized to come from tendon organs, usually showed a higher threshold

to stretch, and their rate of discharge increased during contraction. The action of the muscle spindle was further elucidated by study of the function of the efferent small nerve fibers which make up about one-third of the cat's lumbosacral ventral root. These fibers, as was already known, were found to be exclusively concerned with contraction of the muscle spindles and hence with regulation of the afferent discharge from them. Each muscle spindle apparently receives from three to five such fibers whose effects on afferent output are additive. Apparently, too, branches of these fibers innervate several different muscle spindles. The effect of stimulation of these small efferents is different during rest and during contraction of the main body of muscle. During rest their stimulation produces afferent discharge; during contraction, relatively little, especially when shortening occurs.

Katz (95, 96) recorded potentials from the near vicinity of muscle spindles in frog muscle, apparently end organ potentials. Afferent spikes were preceded by brief step-like prepotentials and followed by a negative afterpotential of several milliseconds' duration. The prepotential usually gave rise to repetitive discharges during stretch of the muscle. This local potential is not abolished by an anesthetic which abolished the spike.When a stretched muscle was released, a transient positive potential occurred.

McCouch, Deering & Ling (125) have located the position and studied the action of the receptors involved in tonic neck reflexes. The receptors lie in the upper joints of the neck, especially the atlanto-axial and atlanto-occipital joints. Their excitatory action is ipsilateral.

The problem of autogenetic inhibition of muscle during strong contraction has been studied by McCouch, Deering & Stewart (126) and also by Granit (67). The former using the knee jerk of decerebrate cats as the indicator, found that it could be inhibited by electrical stimulation of the distal, musculotendinous end of crureus. They concluded that the tendon spindles of Golgi were the source of the inhibition, since no other receptors are known in this region. The entire muscle group was inhibited by tendon spindles in one of the group. Granit found that vigorous stretch or contraction of a component of the quadriceps produced inhibition of the synergistic gastrocnemius (this was preceded by facilitation, as would be expected). Inhibition was favored by high initial tension of the quadriceps. The conduction speed of the inhibitory process was almost as fast as that of the (monosynaptic) excitatory effect.

Central mechanisms.—Bates (14) succeeded in recording cortical activity during muscle contraction. He found, 20 to 40 msec. after the onset of the electromyogram, small potential changes in the contralateral hemisphere, near the central sulcus. Their previous studies of the part played by muscle afferents in changing the excitability of cortical motor points were extended by Gelhorn & Johnson (59). Facilitation and inhibition of response of arm and shoulder muscles to electrical stimulation of the cortex of the monkey were caused by placing the various arm joints (shoulder, elbow, and wrist) in different positions, e.g., acute and obtuse angles, pronation and supination, protraction and retraction.

VESTIBULAR FUNCTIONS

Research on vestibular function has shown considerable change in emphasis. More than half of the recent studies fall in two areas: electrophysiological and physical studies of the peripheral receptors, and drug studies of vestibular reactions or of motion sickness. The former were stimulated by the earlier successes of Lowenstein & Sand, Adrian, and Gernandt; the latter, by the wartime demonstrations of success of motion sickness preventives. There have been numerous studies of the effects of streptomycin on the vestibular system, but except for two references, these are omitted from this review because they have not yet made significant scientific contributions. Wendt's review of vestibular function in the new *Handbook of Experimental Psychology* (166), based on 232 references, covers the journals received in this country through 1948. (Some important European journals were not sent here after 1940.)

Peripheral Mechanisms

Anatomical studies.—Altmann & Waltner (2) injected iron salts into the subarachnoid space and the scala tympani in rabbits, cats, and monkeys. On post-mortem examination of the perilymph, they could detect no iron salts. They concluded that neither the cerebrospinal fluid nor the endolymph is the source of the perilymph but that most likely it is formed locally from blood vessels.

In a review of the comparative anatomy of the semicircular canals and otolith organs, Mygind (132) concluded that the neuroepithelia of both are of the same character i.e., pressure receptors derived from cutaneous end organs. Their differential response to rotary and straight-line forces is determined by their structure. He put forward the somewhat unlikely hypothesis that the observed continuous discharge of the end organs is due to the continuous hydrostatic pressure of the endolymph.

Vilstrup (162, 163, 164), using cod ampullae treated by the Altmann-Gersh freezing-drying method, has studied the embryology and adult status of the sensory epithelium of the ampullar cristae. In the immediate subcupular zone, he found a syncytial structure as part of the sensory cells. Vilstrup put forward the hypothesis that the cupula has its action on the subcupular epithelium chiefly by virtue of a sliding motion rather than by bending or deflection. In a physiological study of living sharks, in rare cases, he was able, by using a glass thread, to get an identical reflex response to cautious touching of the epithelium on either side of the cupula.

Threshold studies.—On the basis of new experiments with 20 humans, Groen (69) modified his earlier value of 0.5° per sec.^{2} as the threshold of perception of rotation. His new figure is 0.25° per sec.^{2} which checks fairly closely with other studies. Using this value to calculate the force acting on the ampulla, he found it to be about the same as the threshold force activating the auditory receptors. Graybiel, Kerr & Bartley (68), using eye movements as the threshold indicator, have further reduced the value of the human

threshold to 0.12° per sec.^{2}. By electrical records from the vestibular nerve of the frog, Ledoux (110) obtained a figure of 2° per sec.^{2} as the threshold.

Action potential studies.—Lowenstein & Roberts (119) extended their laboratory's study of the labyrinth of the thornback ray to include a simultaneous study of utricle, saccule, and lagena. All were found to respond similarly to lateral, to fore and aft tilting, and to linear accelerations, leading to the conclusion that their functions overlap. Ledoux (110, 111, 112) has appeared as a new active investigator of vestibular nerve responses in the frog. In the main, his findings correspond to the well-known results of Lowenstein & Sand, of Adrian, and of Gernandt [see (166)]. There is a spontaneous discharge during rest, inhibited by rotation in one direction and speeded up by rotation in the other. Spontaneous discharge from the utricle is modulated, also, by motion in one direction or the other. According to Ledoux the saccule responded only to vibration (up to 550 c.p.s.), a finding challenged by other work by De Vries *et al.* [see (40) below]. The utricle responded both to vibration and gravity. Both microphonics and action potentials were obtained. Gernandt (60) has now apparently proven, from records of single nerve units in the cat, that the cristae of the semicircular canals respond to small centrifugal forces. This confirmed earlier predictions by Dohlman [see (166)] based on the fact that the specific gravity of the cupula exceeds that of the endolymph.

A major contribution to knowledge of receptor action has been made by De Vries & Bleeker (40). Their contribution, both experimental and theoretical, is based on data from receptor microphonics (the "crista effect") when the labyrinth of the pigeon, after fenestration, was stimulated by sound. Under these conditions, there was a large microphonic potential from the crista of the horizontal canal after destruction of the cochlea. In fact, it was larger than that normally obtained from the cochlea. The crista effect was greatest between 500 and 900 c.p.s. It increased in a linear fashion with sound pressure at low intensities and had not yet leveled off with their most intense tones (100 db.). The microphonics of the utricular crista, on the other hand, leveled off at 70 db. The ampullar crista effect showed the same phase relations to the sound wave on both sides of the ampulla. This was interpreted to mean that the voltages were generated between base and top of the crista. [Van Eyck (156, 157), in an article seen only in abstract, is said to have found opposite phase relations on each side of the cupula in pigeons.] These microphonics are reduced by preceding exposure to sound, and this fatigue shows recovery. The electrical response of the crista was as large or larger than that of the cochlea; however, earlier studies (166) showed that sound produces no vestibular reflex response until it is about 60 db. above threshold.

De Vries & Bleeker (40) suggested that the sense cells in the canals are so built as not to respond readily to alternating currents at high frequencies. The significance of these potentials depends on whether similar potentials are generated during rotary accelerations. A rationale is offered for the

viewpoint that the mechanism of receptor stimulation must be electrical. The threshold of the canals is exceedingly small. If only a mechanical process were involved, the energy would be distributed over a large number of sense cells. According to calculations, each cell would receive an amount of energy less than that of thermal movement of molecules; so it is necessary to assume a mechanism which can concentrate this energy on a few cells. Electrical currents, channelled into paths of low resistance, are such a mechanism.

Mechanical principles of the canals.—Mayne (124), a guided missiles engineer, and De Vries (38), a physicist, have made theoretical and experimental contributions to the theory of receptor action. The former, basing his theory on published work of others, devised equations for action of the receptors. The signals issued by the canal receptors are proportional to the angular velocity of the subject's head between limits of the threshold and some upper limit of linearity and between lower and upper frequency limits. Mayne calculated the magnitude of these limits. De Vries experimentally determined thresholds for perception of rotation (two subjects), using some 7,000 stimuli to find the relation between duration (T) of the acceleration and the minimum perceptible acceleration (a). For each combination of aT, the frequency of perception of rotation was determined. He found the so-called aT-law to be valid only for very brief accelerations of $\frac{1}{4}$ sec. or less. The threshold at 1 sec. required aT to be twice as large as at $\frac{1}{4}$ sec. This implied to De Vries that the excitation of the sense cells does not depend only on the displacement of the cupula.

Regotti & Sala (138) studied the aT relation to the output of vestibular nystagmus (rabbits and humans), using durations from 310 to 670 msec. and a single acceleration, 80° per sec.^{2} As did De Vries, they found that brief, strong accelerations yielded more response for the same aT product than did longer and lesser accelerations.

Van Egmond, Groen & Jongkees (155), members of an active research group in Holland, interpreted the available data on eye movements as showing that the ampulla and endolymph system can be considered as a torsion pendulum with a high degree of damping. It acts as a linear impulsometer so long as accelerations last less than 3 sec. The small deviations from theory were attributed to leakage of fluid between cupula and ampulla.

Using the electrical method of recording eye movements, which depends on the corneoretinal potential difference, Jung (94) plotted the relation of total output of primary nystagmus to velocity of rotation. He claimed that the maximum output was reached at rotation speeds of 60° to 120° per sec. and that the higher speed, 180° per sec., yielded no increment in output. The present reviewer doubts this finding. It is contrary to the results of an as yet unpublished doctoral dissertation done in his own laboratory by Hauty (78a) in which a superior recording method was used and the interfering influence of habituation prevented. Hauty obtained a linear relation of nystagmus output to stimulus input (velocity) over the range 11° per sec. to 180° per sec.

XUM

Otoliths.—DeVries (39) experimentally determined the physical constants (mass, elastic resistance, damping, natural frequency, elastic limits) of the otolith organs of fish. By means of x-ray photographs, he recorded displacement of the otoliths under the influence of gravity, centrifugal force, and vibration. The figures differed considerably for the large otoliths of the saccule and for the smaller ones of the utricle and lagena. De Vries presented the equations which describe these motions.

Jongkees (89) appears to have clarified the function of the saccule in the rabbit. While their removal did not alter behavior, response to linear accelerations directed from one ear to the other was reduced. When a small steel ball was pressed against the receptors in the saccule, vigorous compensatory movements were evoked. A tiny steel ball was implanted and acted on either by a magnet or by acceleration. These procedures, too, produced strong reflex response.

The deaf.—Previous studies [see (166)] showing that about 85 per cent of the congenitally deaf have reduced vestibular function were confirmed by Worchel & Dallenbach (170). Their test, ability to stand on one foot after rotation, has the virtue of simplicity.

Central Mechanisms

There is little that is new in recent studies of central mechanisms. Hamberger (75), however, applied new chemical techniques to tracing vestibular pathways. He claimed to have been able to detect, after brief vestibular stimulation, an increase in nucleoprotein production in the nerve cells of the peripheral vestibular ganglion and within Deiter's nucleus. Acceptance of this finding probably should be withheld pending further study.

Marco (123) made anatomical studies of the vestibular nuclei of the mouse. By means of lesions of Bechterew's nucleus in cats, Owada (133) produced vertical nystagmus. Posterior damage yielded nystagmus with fast phase up; anterior damage, with fast phase down. It appeared that the balanced centers for up and down eye movement are in the front-back axis, while those for horizontal movement are on each side of the neural axis.

Kempinsky & Ward (99) demonstrated that cutting either vestibular nerve in cats markedly reduced the motor response to cortical electrical stimulation. This effect took 4 to 6 min. to appear after section. Removal of the cerebellum did not alter results. According to Liesch (117), the human knee jerk and Achilles tendon jerk are facilitated by rotation.

Action potentials of single units in the inferior colliculus and medial geniculate body (cat) during vestibular stimulation were recorded by Gernandt (61). He concluded that the course of vestibular pathways parallels the acoustic system. Although some of the pathways cross over, it is in the main an uncrossed system. Kempinsky (98) charted the cortical vestibular receiving area by the electrical activity from single shocks to the vestibular nerve in the cat. The cortical area (in the anterior descending limb of the suprasylvian gyrus) overlapped the foreleg and face area in front and the

auditory area in the rear. Penfield & Rasmussen (134) reported that occasionally electrical stimulation of the first temporal convolution in man caused dizziness.

That the postural effects of lesions of the cerebellar cortex are not dependent on the presence of the labyrinths in guinea pig was reported by Manni (122). The effects of cortical ablations on the symptoms produced by cutting one vestibular nerve in guinea pigs has been investigated by Menzio (127). Homolateral lesions, especially in the frontal cortex, were alleged to decrease the symptoms; contralateral lesions, to increase them. Di Giorgio & Pestellini (41) confirmed previous evidence that cerebral cortical and cerebellar lesions, made after habituation to rotation or after recovery from cutting one vestibular nerve, produce a return of response and of symptoms.

Eye Movements

Van Egmond *et al.* (154) and Jongkees (90) discussed methods and presented data on the clinical use of rotation and caloric tests. These are interesting as examples of the convictions of this group of research workers. Koella, Forster & Szabo (104) recorded and Jongkees & Hulk (92) observed the plane of nystagmus when the position of the head with relation to the plane of rotation was systematically varied. An important study of the relation of single ampullae to single eye muscles in the decerebrate cat was reported by Szentágothai (152). He recorded isotonically from all 12 extraocular muscles while stimulating single ampullae. This was done by inserting a fine glass cannula into the canal and then producing endolymph displacement. It seems that there is a predominant connection between any crista and two of the extraocular muscles. The crista of the superior canal is predominantly connected with the ipsilateral superior rectus and the contralateral inferior oblique; the crista of the posterior canal with the ipsilateral superior oblique and the contralateral inferior rectus; and that of the horizontal canal, with the ipsilateral medial rectus and the contralateral lateral rectus. Vilstrup (161) found in the shark a connection of the anterior canal with horizontal deviation of the eyes. He also showed (160) that deviation of the eyes was produced by utricular stimulation.

Knapp (103) reported that 13 of 725 apparently normal people had some degree of spontaneous nystagmus. By both human and animal experiments, de Kleyn (35) concluded that the neural origin of spontaneous "darkness nystagmus" and of nystagmus when lying on one side while intoxicated are the same and are mediated by the same centers as vestibular nystagmus.

Interaction of visual and vestibular control.—Aschan (10) and Jongkees (90) took up again the old problem of whether vision should be excluded in clinical tests of vestibular nystagmus. The former showed experimentally on 100 flying cadets that wearing Frenzel glasses (which blur vision) almost doubled the duration of the observed primary nystagmus. (Neither author suggested that complete exclusion of vision might be an even better tech-

XUM

nique.) Guedry (70) reported that vestibular nystagmus, as shown by the oculogyral movement of a small visual target in the dark, was inhibited by illuminating the room and, further, that a brief period of illumination decreased the total duration of the reported movement of the target. (The latter finding should be checked by graphic records before one concludes that nystagmus is shortened.) An abstract of a study by Sala (146) reported that optokinetic stimulation had no effect on immediately subsequent vestibular nystagmus. He was interested in the neurological implications of this lack of effect and concluded that visually aroused nystagmus did not utilize the vestibular nuclei. Repeating studies done long ago by Dodge (recording nystagmus of humans during simultaneous body rotation and rotation of the visual field), Jung (93) found that under his conditions the visual effect was dominant, being facilitated or reduced by congruent and competing body rotation.

De Kleyn (35) studied interaction of visual and vestibular nystagmus in a five-year-old boy with apparently complete destruction of the cortical visual areas. Visual pursuit and fixation movements were absent but optokinetic nystagmus (believed by De Kleyn to be of subcortical origin) was demonstrated when the visual field was rotated.

Motion Sickness

Excellent reviews of motion sickness, each making original contributions too numerous for inclusion in this review, were published by Tyler & Bard (153), by Birren (21), and by Morales (130). The first is a general review; the second is general, but with an emphasis on psychological factors; and the third deals mainly with physical factors and the motion of ships.

Borison & Wang (23) localized the vomiting center in the medulla of decerebrate cats in the region of the fasciculus and nucleus solatarius and the dorsolateral border of the lateral reticular formation. Electrodes were placed by use of the Horsley-Clarke instrument.

The wartime study by Fraser & Manning (56) of the effect of variation in swinging radius and arc on incidence of sickness in humans has now been published. Fifty subjects were swung at each radius, 6 ft., 10 ft., and 16 ft. and each arc, 50°, 90°, 130°. Sickness increased from 4 per cent to 58 per cent from the 6 ft. to the 16 ft. swing. It increased from 22 per cent at 50° swinging to 50 per cent at 90° swinging. There was no further increase at 130° arc.

Rubin & Winston (143) and Winston *et al.* (169) found that morphine increased nausea and vomiting due to vestibular stimulation. Amphetamine (Benzedrine) did not relieve this, but dimenhydrinate (Dramamine) did. Campbell (29) reported that dimenhydrinate, 600 mg. per day, relieved the vestibular reactions following fenestration. Also, Shaw (149) reported 14 United States Navy studies of use of dimenhydrinate for motion sickness, finding that it had some preventive action in 50 mg. doses, some therapeutic action in 100 mg. doses.

Drug Studies

Winston *et al.* (169) gave 100 mg. of dimenhydrinate to 11 men who were then rotated (with eyes open) and subsequently observed for the duration of postrotation nystagmus. They found no alterations from the normal. Gutner, Gould & Batterman (73) in a similar study of caloric nystagmus after dimenhydrinate medication claimed there was a "marked depression" of nystagmus, while hyoscine was found to increase nystagmus. Neither of these studies is adequate for the conclusions reached because no precautions were taken to avoid the effects of visual stimuli or of relaxation of the subject. The present reviewer doubts the correctness of the conclusions. Fermin, van Deinse & Hammelberg (48, 49), using rabbits, gave very large dimenhydrinate doses (up to 70 mg. per kg.). With increasing doses, the animals became unresponsive first to stimulation of the otoliths and later to stimulation of the canals. Schiff, Esmond & Himwich (147) abolished forced circling movements by dimenhydrinate.

According to an abstract, Camaisasia (28) gave mescaline to guinea pigs before rotation and found that responses were depressed for about 3 hr., but thereafter greatly enhanced. Dederding (34) who gave guinea pigs quinine, pilocarpine, or sodium salicylate reported that the two former drugs caused head deviations and rolling and also an impairment of hearing, whereas only one of four animals showed these symptoms with the last drug. The effects were temporary.

It was found by Riecker (140) that phenobarbital given to normal people causes spontaneous positional nystagmus similar to that produced by alcohol. Modónski (129) confirmed that dihydro-oxycodeinone hydrochloride (Eukodal) causes a spontaneous vertical nystagmus. With both human and animal subjects, Jongkees & Hulk (91) and De Kleyn & van Deinse (36) produced evidence that the damaging effects of streptomycin are localized in the vestibular nuclei.

Hasegawa (78) claimed that he prevented motion sickness by giving sodium bicarbonate. His hypothesis was that it dissolves the otoliths. According to an abstract, Fujisaki (57) gave it to 474 soldiers at sea, but no data are reported in the abstract. He is said to have dissolved the otoliths of frogs and fishes by use of this substance. Giaccai & Carle (62) challenged these statements on the basis of experiments with very high doses given to pigeons and guinea pigs.

Miscellaneous

Arslan (9) reviewed his own early studies and presented new data on autonomic effects of rotation. Simple rotation in one plane does not cause autonomic disturbances, but interrupted rotations or rotations in different planes do. Mann & Passey (121) tilted subjects from 5° to 55° in the lateral plane and maintained this for periods up to 65 sec. prior to asking the subjects to return themselves to the vertical. With increasing amount and with

increasing duration of tilt, there was an increase in the constant error in the direction of the initial tilt. Sacchi (144) found that postrotation nystagmus caused subjects to err in setting a line to the vertical. The upper end of the line appeared to deviate in the direction of the slow phase of nystagmus. According to Arnoult (7), after rotation sound localization is displaced in the direction of the slow phase. The subjects used the hand to point at the sound. Azoy (12) presented norms on 200 subjects on a new tilt table and reviewed the literature of this subject.

CHEMICAL SENSES[4,5]

Taste

Pfaffmann, author of last year's annual review on this subject, has a good section on taste in the new *Handbook of Experimental Psychology* (135).

Anatomical, physiological, and chemical studies.—Wright (172) found, contrary to widespread belief, that the taste buds of the salamander are not dependent on the trophic influence of gustatory nerve fibers for growth or survival. Autoplastic grafts of tongue tip to the enucleated orbit grew and survived 12½ months.

Some of the relations of molecular structure to sweetness have been discussed by Jacobs (86,87). Although saccharine is sweet, a related substance, isomeric with it, is not sweet. (See also sections on theory of olfaction, below.) Dethier (37) has determined taste thresholds of man and blowfly for a series of homologous aliphatic glycols. Threshold concentrations (for both man and blowfly) decreased logarithmically as the glycol carbon chain was lengthened. Von Frisch (165) included a section on chemical senses in a book about bees.

Since monosodiumglutamate has recently been heralded as a remarkable enhancer of food taste, Lockhart & Gainer (118) tried to determine whether its presence affected intensity of sweet or salt. After determining thresholds for glucose, sodium chloride, and monosodiumglutamate (eight subjects), they used the so-called "triangle test" to find whether the alleged enhancement took place. (A standard stimulus is presented, followed by a pair of stimuli from which the subject picks the one that differs from the standard.) Additions of small amounts of monosodiumglutamate to the sugar and salt solutions did not enhance their tastes.

Goetzel, Ahokas & Payne (63), who had found variations in olfactory threshold depending on time since the last meal (see below), reported similar results for taste. Varying concentrations of sucrose were tested

[4] Bernard Weiss, Graduate Assistant at the University of Rochester, collaborated in the preparation of the section on CHEMICAL SENSES.

[5] This review will be limited to taste and smell. Material on other chemoreceptors such as those of the common chemical sense and those in the carotid and aortic sinuses is omitted. A symposium on the latter has appeared recently (158).

(16 subjects) at 10 and 11 A.M., and at 1:30 and 4:30 P.M., on days with and without a noon meal. Thresholds decreased during the morning, increased following lunch, and decreased later in the afternoon. When lunch was omitted, the early afternoon thresholds also decreased. The data show rather large variability; 26 per cent of the days failed to show changes in the expected direction.

As a prior step to studies of consumer preferences (for the U.S. Department of Agriculture), Bayton (16, 17) determined differential thresholds for sweetness of canned orange juice. Juices varying in Brix-acid ratio (a ratio of sugar to acid content) were compared by the triangle test (called "duo-trio test" by Bayton). The obtained thresholds were much larger than had been anticipated.

Taste blindness continues to be a field of discussion and investigation. Cardullo & Holt (30) have shown that the tasting or not tasting of phenylthiocarbamide can be tested in early infancy and suggested use of such evidence in cases of questioned paternity. Boyd (27) found a natural substance, (1,5-vinyl-2-thio-oxazolidone), sensitivity to which parallels sensitivity to the synthetic phenylthiocarbamide. In 7 nontasters, 13 tasters, and 1 taster with delayed reaction, the results with both substances were the same. The natural substance occurs in turnips, cabbage, and many other plants. Boyd has also written a discussion article about taste blindness (26) and a book on genetics (25) containing material on phenylthiocarbamide. In the first, he reaffirmed the validity of the evidence for sex and race differences. The second is a general review.

Macht (120) studied cats with the neural axis transected to cut off the higher centers. Rejection thresholds for hog liver dipped in quinine, in saline solution, and in acetic acid seemed to coincide with those of normal cats. Liver dipped in sucrose was not rejected. Responses of the sweet taste fibers of the dog have been studied by Andersson, Landgren, Alsson & Zotterman (4). Action potentials were recorded from strands of the peripheral part of the lingual nerve and the central portion of the chorda tympani. Solutions applied to the side of the tongue tip evoked impulses which, they believed, probably travel over small fibers of 4 to 6 μ. Lingual nerve preparations also responded to cold. Feeble discharges were recorded from the chorda tympani during mechanical or cold stimulation; this probably shows that it is not exclusively a taste nerve. Chorda tympani preparations responded well to solutions of cane sugar, glycerine, ethylene glycol, and lead acetate. Saccharine solutions produced no large spikes, even in preparations responding to sugar. Thus, in the dog, saccharine does not excite the same receptors as sugar. On the other hand, response to a bitter substance was so similar in respect to fiber diameter involved and in type of response that it was surmised that saccharine gives rise to a bitter taste in dogs. Pfaffmann & Bare's study (136) showed that the threshold of receptors responding to saline does not change in adrenalectomized rats. In their experiments on the human cerebral cortex, Penfield & Rasmussen (134) never got taste

reports from stimulation of the sensory tongue areas. Taste was occasionally reported, however, during stimulation deep in the fissure of Sylvius where it joins the island of Reil. The authors consider the data too meager to justify localization of taste points.

Two studies concern self-selection of diet. Randoin, Causeret & Gavrel-Szmanski (137) found that young rats showed an initial preference for weak saline solutions rather than plain water. After three or four weeks, however, the consumption of plain water rose until it equaled or exceeded that of salt water. The authors hypothesized that the initial preference is without physiological significance. Richter (139) reported an experiment on rats and humans, carried out with 11 different posions to find out whether all toxic substances have a "warning" taste. Rejection concentrations were determined for 70 rats. The 60 human subjects merely tasted the solutions. The results showed that the rats and humans did not differ widely in taste ability and that some highly toxic substances have no taste to rats or men. Bayton (15) has reviewed food preference studies being done by the U.S. Department of Agriculture.

Sheffield & Roby (150) found that saccharine, although nonnutritive, acts as a reward for rats in instrumental learning. Hungry, thirsty rats consumed more saccharine solution than satiated, thirsty rats in a preference situation. The maze was learned as quickly with saccharine solution as the reward as with food.

Olfaction

General articles.—Most of the year's publications on olfaction relate to the peripheral mechanisms of stimulation of the olfactory receptors, approached either by experiment or theory. Three different symposia on olfaction were held under the following auspices: the Society of Cosmetic Chemists of London (6), the Scientific Section of the Toilet Goods Association (52, 82, 128, 145, 167), and the Office of Naval Research (unpublished).

The papers at the symposium of the Toilet Goods Association were very worthwhile. Two of these will be mentioned at this point. Sagarin (145) was sharply critical of the scientific standards in this field which he regarded as cluttered with erroneous notions that were accepted, not on evidence, but merely on the fact of a publication. The history of an alleged relation between albinism and anosmia was cited as an example. It had for a number of years been widely believed that albinos have no sense of smell. Foster finally disproved this. Sagarin searched for the source of the belief. He traced it to a single, physician's report of a Negro patient who lost his skin pigmentation and simultaneously lost smell sensitivity! Olfactory research, Sagarin stated, can be put on a scientific footing only by repetition of experiments in critical areas, by use of reproducible experimental conditions, and by use of large numbers of subjects. The last admonition, repeated by others at the symposium, was based on the unusually large individual differences in this sense field. Foster's symposium paper (52) on future pathways of

olfactory research is a useful statement of the 12 major problem areas, their present status, and the methods necessary for their advancement.

The most elaborate olfactory laboratory yet constructed, the Cornell Olfactorium, has been described by Foster, Scofield & Dallenbach (54). It consists of two units: two glass chambers (an antechamber and an experimental chamber) and an air treatment system. To achieve a stream of odorless, air, incoming air undergoes the following treatment: mechanical filtering to remove gross impurities, moistening with a water spray, removal of excess water in a series of baffles, passage through a radiation heater, passage through 12 cannisters of activated charcoal, and, to remove the charcoal odor, passage over a bed of silica gel. To aid in retaining freedom from odor, the subject is covered with a plastic envelope which exposes only his face. Odors are introduced by means of an aluminum compartment placed in the air stream. A double atomizing unit within the compartment presents single or mixed odors. Up to five vials can be placed in the same compartment, and the air stream passed over them. Wenzel (167) urged not only standardization of procedure but also early production of handbook data on absolute and differential thresholds for olfactory stimuli.

Peripheral mechanisms: theory and experiment.—Middleton's review of theories of olfaction (128) found no support for direct radiation theories and difficulties with emanation theories based on the chemical reactivity of diffused odorous material. He found infrared theories to be untenable, because of the small physical dimensions of the receptive surface, yet felt that tests of the theory will be productive in a supporting role. The most probable theories are those relating smell to the unit of odor, to the molecule, and to the phenomena occurring at surfaces. Such processes as electrical charges, permeability and surface tension changes, distortions of minute dimensions, and physical displacement tendencies of one type of molecule or force on another were suggested. The "site-filling" theory of Moncrieff, postulating, as it does, that the molecules of the receptors have surface structures into which the structures of odorous molecules can fit, was looked on with approval. For a critical test of this promising lead one must await further knowledge of the structure of large molecules, an area in which research results are slow in arriving because of the extreme difficulty of the techniques. Beets (19) strongly objected to Moncrieff's theory of a relation between unsaturation, resonance, and odor on the ground that the alleged increase in molecule flexibility with resonance and unsaturation is not true.

The Beck-Miles infrared theory which involved the transient cooling of the receptors, as a result of selective radiation to the odorant molecules, has been modified by Beck (18) because it did not fit the evidence. He is now convinced that odorous substances form surface films on the receptor surface and that this film interferes with radiation and gives varying patterns of cooling, depending on the substance. He reinterpreted old experiments in the light of this theory. According to an abstract, Fortunato (50) has further evidence against the Beck-Miles theory.

XUM

Lauffer (108) has developed a more sophisticated theory of the action of molecular configuration than the "site-filling" theory of Moncrieff (which holds that the molecules of the receptor elements have slots into which the odorant molecules fit and vice versa, something like pieces of a jig-saw puzzle). Lauffer surveyed nonolfactory biological processes, particularly from the standpoint of biochemistry, to find mechanisms which might fit the requirements of the olfactory process. He suggested that enzyme action is a likely candidate. Enzymes, like all proteins, are macromolecules that form addition compounds by means of minor attractive forces, such as hydrogen bonds, which may also determine their shape and specific activity. The weakness of these forces makes it necessary that a molecule fit closely against the enzyme over an extensive area for the addition compound to be formed. The molecule-enzyme alliance, Lauffer suggested, stretches or unfolds the molecule so that points normally unexposed may become capable of chemical reaction. Thus, if the molecules of the odorant produce their effects by combining with protein molecules (enzymes), the importance of molecular shape is clear. He also suggested that it is not the shape of the entire molecule that may be important, but only that part which fits against the enzyme. The action by which excitation is produced, he felt, is interference with some vital process by alteration of the enzyme or enzymes concerned.

Kistiakowsky (102) has also proposed an enzyme theory of smell, and El-Baradi & Bourne (45) have independently developed the same kind of theory. The latter have experimental evidence for its applicability to olfaction and especially to taste. It is assumed that there are several active enzymes present in the olfactory epithelium and in the epithelium around the gustatory cells. Odorous or taste compounds have the ability to inhibit these enzymes. This selective inhibition alters the relative concentration of various compounds at the receptor and initiates the neural response. El-Baradi & Bourne showed that alkaline phosphatase is present in the olfactory and gustatory epithelia and that it may be inhibited by vanillin, tea infusion with capricum, coffee, oil of aniseed, and oil of peppermint. Sugar, salt, and quinine did not inhibit it. The presence of a simple esterase and its inhibition by quinine, but not by sugar or salt, were also shown.

Le Magnen (116) has experimental evidence for the rat that odors are distinguished by the spatial configuration of the molecule rather than by their chemical constitution. He used 11 pairs of odors to be discriminated. These substances were adsorbed on the glass mouthpiece of the rats' water bottles. Each rat was trained for 10 days on each odor pair, using strong quinine solution in one bottle and sucrose in the other. In the test series, the odor pair remained the same but plain water was used in each bottle. The ratio of consumption of water from the two bottles was the measure of odor discrimination. Guillot & Thibaut (72) have obtained comparable evidence on humans' ability to distinguish odors on the basis of molecular structure, by using the optically inverse forms of isoborneal. Their results

seem to confirm Beck's contention that the molecular components of a complex odor can be "sniffed out."

A related experimental approach is that of studying the effect of alterations in body chemistry on smell sensitivity. Guillot (71), using one subject, took smell thresholds for isovaleric acid before and after injection of the specific antibodies for this substance. The disperion of the thresholds was judged to be the same under both conditions; so he concluded that no change in sensitivity occurred. Since this involved but one subject, one should regard the conclusion as not yet proved. Le Magnen (115) claimed to have shown very large changes in sensitivity to a musk-like odor (exaltolide) in women as a function of sex hormones. At the time of ovulation, sensitivity was greatest, having increased over a period of 7 to 10 days to a value 100 to 100,000 times that present during menstruation. Six ovariectomized women showed increases when treated with estrogen. Goetzel, Abel & Ahokas (64) studied diurnal variations in olfactory sensitivity (Elsberg apparatus) as related to mealtime. Olfactory acuity was reported to be greater shortly before than shortly after eating a normal midday meal. This experiment lacks most of the essential controls, and the conclusions should not be accepted until it is repeated under adequate conditions. [A poorly-written abstract of a paper by Barát (13) seems to imply that he found maximum olfactory acuity in women just before menstruation.] A number of experiments were reported by Westcott (168) which suggested to the investigator that chlorophyllin reduces body odors and breath odors. This has relevance to olfaction because, in a number of instances, the subjects were asked to determine improvement in themselves. If such techniques were used, the study is small support for the current chlorophyll deodorant fad.

The classical approach to odor theory, that of classifying odors, is represented by informal reports of two experiments in progress. Duncan [in (6)], using a technique identical with "independence threshold," but which he called the method of odor fatigue, has obtained results that suggest the presence of a large, though limited number of primary odors. This promises to be a useful method, since it leads to classifications on the basis of common action on the receptor rather than on a commonality of verbal responses. It is reported (5) that Cheeseman is carrying out work on odor classification by means of factor analysis.

Foster & Scofield (53) read a paper on odor mixture, describing experiments in which two, three, or four odors were presented together. It was found that odor mixtures are generally distinct from the odors of their components.

Neural mechanisms.—A very important paper has been contributed by Adrian (1), summarizing what he has learned about the electrical activity of the olfactory bulb. From different layers of the bulb arise different types of activity: spikes in white matter and wave-like potential oscillations near and on the surface. The latter waves are of two kinds: induced waves set up by strong olfactory stimuli and intrinsic waves caused by the persistent

activity of cells in the bulb (cells with short axons, not the mitral cells). In deep anesthesia, the intrinsic waves are suppressed and the olfactory signals are transmitted without interference. In moderate anesthesia, the intrinsic activity becomes continuous and prevents the transmission of signals from the receptors. As the anesthesia becomes lighter, the olfactory signals again gain control to some extent; they disorganize the rapid intrinsic rhythm and suppress it in favor of olfactory discharge at each inspiration. Ultimately, however, the intrinsic activity builds up again and the persistent discharge returns. As there was no sign of failure of the receptors under repeated stimulation, Adrian gave a central explanation for smell adaptation, attributing it to the blotting out of receptor signals by the reappearance of the intrinsic waves.

On the basis of new studies, Le Gros Clark (113, 114) has modified his earlier claim that there is no localization in the projection of the olfactory epithelium to the bulb. Adrian's electrophysiological evidence for some localization led to extension of histological work to the entire epithelium. The new study in the rabbit yielded results which indicate a degree of regional projection of the olfactory epithelium on the olfactory bulb.

Another anatomical study yielding new facts is that of Larsell (107). He demonstrated afferent terminations of a sensory component of nervus terminalis in the olfactory epithelium. Larsell speculated that these fibers synapse on efferents returning in the same nerve, which activate Bowman's glands. Odorous stimulation thus effects a release of fluid from the glands, and the olfactory receptors are stimulated by changes in the fluid or by saturation of the fluid by the odorant.

Frankenhauser (55) reported the curious result that he was unable to record strychnine spikes in the tracts of the olfactory bulb in cats. He concluded that strychnine does not activate the cells of the bulb so as to set up spikes. Erhart (46) found no significant racial differences in the structure of the olfactory trigone, in the lateral and medial olfactory gyri and striae, and in the diagonal band. The paraolfactory prominence was found significantly more frequently in whites (50 whites and 50 Negroes or mulattoes compared) than in Negroes.

Miscellaneous.—Foster (51) investigated variations in pleasantness-unpleasantness characteristics of odors in relation to age. Using both familiar and unfamiliar odors on five age levels, he found that children and adults tend to accept and reject the same odors. No universally accepted or rejected odor was found. Morlan (131) studied the ability to distinguish body odor of two whites and two Negroes (immediately after bathing and after exercise). Slightly better than chance performance was found. Hersh (82) reviewed olfactory abnormalities.

LITERATURE CITED

1. Adrian, E. D., *EEG Clin. Neurophysiol.*, **2**, 377–88 (1950)
2. Altmann, F., and Waltner, J. G., *Ann. Otol. Rhinol. Laryngol.*, **59**, 657–86 (1950)
3. Anderson, A. B., and Munson, W. A., *J. Acoust. Soc. Am.*, **23**, 155–59 (1951)
4. Andersson, B., Landgren, S., Alsson, L., and Zotterman, V., *Acta Physiol. Scand.*, **21**, 105–19 (1950)
5. Anonymous, *Am. Perfumer Essent. Oil. Rev.*, **50**, 473–74 (1950)
6. Anonymous, *Nature*, **165**, 394–95 (1950)
7. Arnoult, M. D., *Am. J. Psychol.*, **63**, 229–36 (1950)
8. Arshavskaia, É. I., *J. Physiol. U.S.S.R.*, **36**, 333–41 (1950)
9. Arslan, M., *Acta Oto-Laryngol. Suppl.*, **78**, 45 (1948)
10. Aschan, G. von, *Acta Oto-Laryngol.*, **36**, 1–5 (1948)
12. Azoy, *Rev. espaň. oto-neuro-oftalmol. neurocirurg.*, **5**, 255–93 (1948)
13. Barát, T., *Orvosi Hetilap*, **90**, 539–41 (1949)
14. Bates, J. A. V., *J. Physiol.*, **113**, 240–57 (1951)
15. Bayton, J. A., *Agr. Econ. Research*, **2**, 105–12 (1950)
16. Bayton, J. A., *U. S. Dept. Agr. Bur. Agr. Econ.* (November, 1950)
17. Bayton, J. A., *U. S. Dept. Agr. Bur. Agr. Econ.* (January, 1951)
18. Beck, L. H., *Med. Physics*, **2**, 658–64 (1950)
19. Beets, G. J., *Am. Perfumer Essent. Oil Rev.*, **55**, 290 (1950)
20. Bing, H. I., and Skouby, A. P., *Acta Physiol. Scand.*, **21**, 286–302 (1950)
21. Birren, J. E., in *Human Factors in Undersea Warfare*, Chap. 18, 375–98 (Natl. Research Council, Washington, D. C., 541 pp., 1949)
22. Birren, J. E., Schapiro, H. B., and Miller, J. H., *J. Pharmacol. Exptl. Therap.*, **100**, 67–71 (1950)
23. Borison, H. L., and Wang, S. C., *J. Neurophysiol.*, **12**, 305–13 (1949)
24. Bors, E., *Arch. Neurol. Psychiat.*, **63**, 713–18 (1950)
25. Boyd, W. C., *Genetics and the Races of Man* (Little, Brown & Co., Boston, 453 pp., 1950)
26. Boyd, W. C., *Psychol. Bull.*, **48**, 71–74 (1951)
27. Boyd, W. C., *Science*, **112**, 153 (1950)
28. Camisasia, L., *I'Oto-Rino-Laring. Ital.*, **18**, 90–106 (1949)
29. Campbell, E. N., *Laryngoscope*, **59**, 1261–72 (1949)
30. Cardullo, H. M., and Holt, L. E., Jr., *Proc. Soc. Exptl. Biol. Med.*, **76**, 589–92 (1951)
32. Chapman, W. P., Rose, A. S., and Solomon, H. C., *Am. J. Psychiat.*, **107**, 221–24 (1950)
33. Clausen, J., and King, H. E., *J. Psychol.*, **30**, 299–306 (1950)
34. Dederding, D., *Acta Oto-Laryngol. Suppl.*, **74**, 145–49 (1948–49)
35. de Kleyn, A. P. H. A., *Acta Oto-Laryngol. Suppl.*, **76**, 8–13 (1948)
36. de Kleyn, A. P. H. A., and van Deinse, J. B., *Acta Oto-Laryngol.*, **38**, 1–7 (1950)
37. Dethier, V. G., *Am. J. Physiol.*, **165**, 247–50 (1951)
38. De Vries, H., *Acta Oto-Laryngol.*, **37**, 218–29 (1949)
39. De Vries, H., *Acta Oto-Laryngol.*, **38**, 262–73 (1950)
40. De Vries, H., and Bleeker, J. D. J. W., *Acta Oto-Laryngol.*, **37**, 298–306 (1949)
41. Di Giorgio, A., and Pestellini, G., *Arch. fisiol.*, **48**, 86–108 (1949)
42. Ebaugh, F. G., Jr., Bird, R. M., and Hardy, J. D., *Proc. Soc. Exptl. Biol. Med.*, **74**, 844–45 (1950)

XUM

43. Ebaugh, F. G., and Thauer, R., *J. Applied Physiol.*, **3**, 173–82 (1950)
44. Edwards, W., *Psychol. Bull.*, **47**, 449–75 (1950)
45. El-Baradi, A. F., and Bourne, G. H., *Science*, **113**, 660–61 (1951)
46. Erhart, E. A., *J. Comp. Neurol.*, **93**, 297–311 (1950)
47. Fedotov, Ĭ. P., *J. Physiol. U.S.S.R.*, **36**, 326–33 (1950)
48. Fermin, H., van Deinse, J. B., and Hammelburg, E., *Acta Oto-Laryngol.*, **38**, 543–49 (1950)
49. Fermin, H., van Deinse, J. B., and Hammelburg, E., *Nederland. Tijdschr. Geneesk.*, **4**, 3036–41 (1950)
50. Fortunato, V., *Clin. Otolaryngol.*, **2**, 61–66 (1950)
51. Foster, D., *Drug & Cosmetic Ind.*, **67**, 46 (1950)
52. Foster, D., *Proc. Sci. Sect. Toilet Goods Assoc.*, No. 14, 14–16 (1950)
53. Foster, D., and Scofield, E. H., *Am. Psychol.*, **5**, 244 (1950)
54. Foster, D., Scofield, E. H., and Dallenbach, K. M., *Am. J. Psychol.*, **63**, 431–40 (1950)
55. Frankenhauser, B., *J. Neurophysiol.*, **14**, 73–79 (1951)
56. Fraser, A. M., and Manning, G. W., *J. Applied Physiol.*, **2**, 580–84 (1950)
57. Fujisaki, S., *Nippon Igaku Zasshi K. K.*, **21**, 14–16 (1949)
58. Gaensler, E. A., *J. Clin. Invest.*, **30**, 406–20 (1951)
59. Gellhorn, E., and Johnson, D. A., *Brain*, **73**, 513–31 (1950)
60. Gernandt, B. E., *Acta Physiol. Scand.*, **21**, 61–72 (1950)
61. Gernandt, B. E., *Acta Physiol. Scand.*, **21**, 73–81 (1950)
62. Giaccai, F., and Carle, A., *Arch. Otolaryngol.*, **53**, 434–38 (1951)
63. Goetzel, F. R., Ahokas, A. J., and Payne, J. G., *J. Applied Physiol.*, **2**, 619–26 (1950)
64. Goetzel, F. R., Abel, M. S., and Ahokas, A. J., *J. Applied Physiol.*, **2**, 553–62 (1950)
65. Gorrell, R. L., *J. Am. Med. Assoc.*, **142**, 557–61 (1950)
66. Graham, D. T., Goodell, H., and Wolff, H. G., *J. Clin. Invest.*, **30**, 37–49 (1951)
67. Granit, R., *J. Neurophysiol.*, **13**, 351–72 (1950)
68. Graybiel, A., Kerr, W. A., and Bartley, S. H., *Am. J. Psychol.*, **61**, 21–36 (1948)
69. Groen, J. J., *Nederland. Tijdschr. Geneesk.*, **1**, 745 (1948)
70. Guedry, F. E., *J. Gen. Psychol.*, **43**, 313–22 (1950)
71. Guillot, M., *Compt. rend.*, **232**, 1249–51 (1951)
72. Guillot, M., and Thibaut, P., *Compt. rend.*, **232**, 1138–40 (1951)
73. Gutner, L. B., Gould, W. J., and Batterman, R. C., *Arch. Otolaryngol.*, **53**, 308–15 (1951)
74. Habgood, J. S., *J. Physiol.*, **111**, 195–213 (1950)
75. Hamberger, C. A., *Acta Oto-Laryngol. Suppl.*, **78**, 55–65 (1948)
76. Hardy, J. D., *Ann. Rev. Physiol.*, **12**, 119–44 (1950)
77. Hardy, J. D., Wolff, H. G., and Goodell, H., *J. Clin. Invest.*, **29**, 115–40 (1950)
78. Hasegawa, T., *Arch. Otolaryngol.*, **50**, 708–15 (1949)
78a. Hauty, G. T., *The Quantitative Characteristics of Primary Ocular Nystagmus as a Function of Acceleration and of Speed of Rotation* (Doctoral thesis, Univ. of Rochester, 1950)
79. Hazouri, L. A., and Mueller, A. D., *Arch. Neurol. Psychiat.*, **64**, 607–13 (1950)
80. Hensel, H., *Arch. ges. Physiol.* (*Pflügers*), **252**, 146–64 (1950)
81. Herman, M., *Arch. Neurol. Psychiat.*, **64**, 42–56 (1950)
82. Hersh, J. H., *Proc. Sci. Sect. Toilet Goods Assoc.*, No. 14, 10–11 (1950)

83. Hunt, C. C., and Kuffler, S. W., *J. Physiol.*, **113**, 283–97 (1951)
84. Hunt, C. C., and Kuffler, S. W., *J. Physiol.*, **113**, 298–315 (1951)
85. Jackson, I. J., *Diseases Nervous System*, **12**, 10–12 (1951)
86. Jacobs, M. B., *Am. Perfumer Essent. Oil Rev.*, **57**, 49–50 (1951)
87. Jacobs, M. B., *Am. Perfumer Essent. Oil Rev.*, **57**, 129 (1951)
88. Jenkins, W. L., in *Handbook of Experimental Psychology*, Chap. 30, 1172–90 (Stevens, S. S., Ed., John Wiley & Sons, Inc., New York, 1436 pp., 1951)
89. Jongkees, L. B. W., *Acta Oto-Laryngol.*, **38**, 18–26 (1950)
90. Jongkees, L. B. W., *Arch. Otolaryngol.*, **49**, 594–608 (1949)
91. Jongkees, L. B. W., and Hulk, J., *Acta Oto-Laryngol.*, **38**, 225–32 (1950)
92. Jongkees, L. B. W., and Hulk, J., *Acta Oto-Laryngol.*, **38**, 274–80 (1950)
93. Jung, R., *Acta Oto-Laryngol.*, **36**, 199–202 (1948)
94. Jung, R., *Klin. Wochschr.*, **26**, 513–21 (1948)
95. Katz, B., *J. Physiol.* (*London*), **111**, 248–60 (1950)
96. Katz, B., *J. Physiol.* (*London*), **111**, 261–82 (1950)
97. Keats, A. S., Beecher, H. K., and Mosteller, F. C., *J. Applied Physiol.*, **3**, 35–44 (1950)
98. Kempinsky, W. H., *J. Neurophysiol.*, **14**, 203–10 (1951)
99. Kempinsky, W. H., and Ward, A. A., *J. Neurophysiol.*, **13**, 295–304 (1950)
100. Kennard, M. A., *J. Neurophysiol.*, **13**, 215–22 (1950)
101. King, H. E., Clausen, J., and Scarff, J. E., *J. Nervous Mental Diseases*, **112**, 93–96 (1950)
102. Kistiakowsky, G. B., *Science*, **112**, 154–55 (1950)
103. Knapp, E., *Hals Nasen Ohrenheilk.*, **2**, 17–19 (1950)
104. Koella, W., Forster, G., and Szabo, T., *Helv. Physiol. Pharmacol. Acta*, **8**, 297–304 (1950)
105. Krayenbuhl, H., and Stoll, W., *Rev. neurol.*, **83**, 40–41 (1951)
106. Kuhn, R. A., and Bromiley, R. B., *J. Pharmacol. Exptl. Therap.*, **101**, 47–55 (1951)
107. Larsell, O., *Ann. Otol. Rhinol. & Laryngol.*, **59**, 414–38 (1950)
108. Lauffer, P. G., *Drug. & Cosmetic Ind.*, **67**, 326–27, 410–17 (1950)
109. Le Beau, J., *J. Neurosurg.*, **7**, 79–91 (1950)
110. Ledoux, A., *Acta Oto-Laryngol. Suppl.*, **78**, 46–54 (1948)
111. Ledoux, A., *Acta Oto-Laryngol.* (*Belg.*), **3**, 335–48 (1949)
112. Ledoux, A., *Acta Oto-Laryngol.* (*Belg.*), **4**, 229–32 (1950)
113. Le Gros Clark, W. E., *J. Neurosurg. Psychiat.*, **14**, 1–10 (1951)
114. Le Gros Clark, W. E., *Nature*, **165**, 452–53 (1950)
115. Le Magnen, J., *Compt. rend.*, **230**, 1103–5 (1950)
116. Le Magnen, J., *Compt. rend. soc. biol.*, **144**, 1319–21 (1950)
117. Liesch, E., *Boll. malattie orecch. gola naso*, **65**, 119 (1947)
118. Lockhart, E. E., and Gainer, J. M., *Food Research*, **15**, 459–64 (1950)
119. Lowenstein, O., and Roberts, T. D., *J. Physiol.* (*London*), **110**, 392–415 (1949)
120. Macht, M. B., *Federation Proc.*, **10**, 88 (1951)
121. Mann, C. W., and Passey, G. E., *J. Exptl. Psychol.*, **41**, 108–13 (1951)
122. Manni, E., *Arch. fisiol.*, **50**, 110–22 (1950)
123. Marco, J., *Ann. d'oto-laryngol.*, **66**, 630–46 (1949)
124. Mayne, R., *J. Comp. Physiol. Psychol.*, **43**, 309–19 (1950)
125. McCouch, G. P., Deering, I. D., and Ling, T. H., *J. Neurophysiol.*, **14**, 191–95 (1951)

XUM

126. McCouch, G. P., Deering, I. D., and Stewart, W. B., *J. Neurophysiol.*, **13,** 343–49 (1950)
127. Menzio, P., *Arch. fisiol.*, **50,** 97–104 (1951)
128. Middleton, J. W., *Proc. Sci. Sect. Toilet Goods Assoc.*, No. 14, 4–9 (1950)
129. Modónski, J., *Acta Oto-Laryngol.*, **37,** 116–23 (1949)
130. Morales, M. F., in *Human Factors in Undersea Warfare* (Natl. Research Council, Washington, D. C., 541 pp., 1949)
131. Morlan, G. K., *J. Genetic Psychol.*, **77,** 257–65 (1950)
132. Mygind, S. H., *Acta Oto-Laryngol. Suppl.*, **70,** 1–114 (1948)
133. Owada, K., *Nippon Igaku Zasshi K. K.*, **21,** 7–17 (1949)
134. Penfield, W., and Rasmussen, T., *The Cerebral Cortex of Man* (The Macmillan Company, New York, 248 pp., 1950)
135. Pfaffmann, C., in *Handbook of Experimental Psychology*, Chap. 29, 1143–71 (Stevens, S. S., Ed., John Wiley & Sons, Inc., New York, 1436 pp., 1951)
136. Pfaffmann, C., and Bare, J. K., *J. Comp. Physiol. Psychol.*, **43,** 320–24 (1950)
137. Randoin, L., Causeret, J., and Gavrel-Szmanski, M., *J. physiol.*, **42,** 447–50 (1950)
138. Regotti, S., and Sala, O., *Sistema Nervoso.*, **1,** 30–44 (1949)
139. Richter, C. P., *J. Comp. Physiol. Psychol.*, **43,** 358–74 (1950)
140. Riecker, O. E., *Z. Laryngol.-Rhinol.-Otol.*, **28,** 138–50 (1949)
141. Rosenthal, S. R., *Proc. Soc. Exptl. Biol. Med.*, **74,** 167–70 (1950)
142. Rowe, S. N., and Moyer, J. B., *J. Neurosurg.*, **7,** 121–26 (1950)
143. Rubin, A., and Winston, J., *J. Clin. Invest.*, **29,** 1261–66 (1950)
144. Sacchi, U., *Sistema Nervoso.*, **1,** 43–50 (1949)
145. Sagarin, E., *Proc. Sci. Sect. Toilet Goods Assoc.*, No. 14, 3–4 (1950)
146. Sala, O., *L'Oto-Rino-Laring. Ital.*, **17,** 341–51 (1948)
147. Schiff, M., Esmond, W. G., and Himwich, H. E., *Arch. Otolaryngol.*, **51,** 672–77 (1950)
148. Schwartz, A. M., Sata, W. K., and Laszlo, D., *Science*, **111,** 310–11 (1950)
149. Shaw, C. C., *Military Surgeon*, **106,** 441–49 (1950)
150. Sheffield, F. D., and Roby, T. B., *J. Comp. Physiol. Psychol.*, **43,** 471–81 (1950)
151. Smolik, E. A., *Diseases Nervous System*, **11,** 327–31 (1950)
152. Szentágothai, J., *J. Neurophysiol.*, **13,** 395–407 (1950)
153. Tyler, D. B., and Bard, P., *Physiol. Revs.*, **29,** 311–69 (1949)
154. Van Egmond, A. A. J., Groen, J. J., Hulk, J., and Jongkees, L. B. W., *J. Laryngol. Otol.*, **63,** 306–10 (1949)
155. Van Egmond, A. A. J., Groen, J. J., and Jongkees, L. B. W., *J. Physiol. (London)*, **110,** 1–17 (1949)
156. Van Eyck, M., *Acta Oto-Rhino-Laryngol.*, **3,** 459–66 (1949)
157. Van Eyck, M., *Arch. intern. physiol.*, **57,** 102–5 (1949)
158. Various, *Acta Physiol. Scand.*, 22 (1950)
159. Vignes, H., *J. psychol.*, **43,** 367–71 (1950)
160. Vilstrup, T., *Acta Oto-Laryngol.*, **38,** 62–67 (1950)
161. Vilstrup, T., *Acta Oto-Laryngol.*, **38,** 68–73 (1950)
162. Vilstrup, T., *Ann. Otol. Rhinol. & Laryngol.*, **59,** 19–45 (1950)
163. Vilstrup, T., *Ann. Otol. Rhinol. & Laryngol.*, **59,** 46–71 (1950)
164. Vilstrup, T., *J. Laryngol. Otol.*, **64,** 420–26 (1950)
165. von Frisch, K., *Bees: Their Vision, Chemical Senses, and Language* (Cornell University Press, Ithaca, N. Y., 119 pp., 1950)

166. Wendt, G. R., in *Handbook of Experimental Psychology*, 1191–1223 (Stevens, S. S., Ed., John Wiley & Sons, Inc., New York, 1436 pp., 1951)
167. Wenzel, B. M., *Proc. Sci. Sect. Toilet Goods Assoc.*, No. 14, 11–13 (1950)
168. Westcott, H. F., *N. Y. State J. Med.*, **50**, 698–700 (1950)
169. Winston, J., Rubin, A., Lewis, J. S., and Rehberger, J. M., *Ann. Otol. Rhinol. & Laryngol.*, **59**, 622–27 (1950)
170. Worchel, P., and Dallenbach, K. M., *Am. J. Psychol.*, **63**, 161–75 (1950)
171. Wright, G. H., *J. Physiol.*, **112**, 344–58 (1951)
172. Wright, M. R., *Proc. Soc. Exptl. Biol. Med.*, **76**, 462–63 (1951)

INDIVIDUAL DIFFERENCES[1]

By Lloyd G. Humphreys
School of Education, Stanford University, Stanford, California

This review is not intended to be comprehensive. Foreign language journals are not covered. The remaining journals, for several reasons, are not thoroughly reported. In part, it is difficult to delimit the subject matter of individual differences for purposes of adequate coverage. In part, it is more interesting for the writer to stress opinion, even though complete coverage of the year's research must be sacrificed.

This review is organized about a selected list of problems in research on individual differences. In discussing these problems, attention will sometimes be given to research that should have taken place during the year as well as to published research results. The writer's aim is to sketch in the outline of a functional psychology of individual differences.

The Nature-Nurture Controversy

There are signs that the nature-nurture controversy is being increasingly avoided and that investigators are defining their problems in a fashion that will lead to clear-cut experimental answers. It is obviously impossible to obtain the degree of control necessary to answer the questions that once were posed in this area. Control is lacking primarily because of the nature of the growth process, involving in the human a long period of infancy and dependency. Adequate controls are possible in certain circumstances, but would involve the sort of juggling with human lives that we find inacceptable, i.e., to take a thousand pairs of identical twins away from their parents at birth, to separate them, and to send them at random into foster homes also selected at random is unthinkable. It is better to ask questions to which answers can be obtained than to argue interminably about unanswerable questions.

Tests as measures of innate capacities.—Let us consider some of the difficulties that arise when attention is focussed on the nature-nurture problem. From the inception of the intelligence testing movement, there has been heated controversy concerning the extent to which the tests measure innate capacity. At the present time, the outstanding critic of these tests on this issue is Davis (14, 15), who is concerned about social class discriminations. To find social class differences is easy; to explain them is difficult. The hereditarian can produce as good an explanation as can the environmentalist.

Davis, as a representative of the latter group, makes the mistake of rejecting present tests in favor of a culturally unbiased test which he is constructing without looking at what present tests can accomplish. The writer

[1] The survey of the literature to which this review pertains was completed about July, 1951.

is a little concerned that a sociologist would attempt to construct a culturally unbiased test, unless he realizes that a test can be unbiased only in the areas in which control is exercised, i.e., by arbitrarily selecting items on which differences between specific groups are not observed. The writer is even more concerned that tests should be judged on evidence that is actually peripheral to their purpose, which is prediction of future behavior.

The hereditarian frequently makes just as serious an error. After he decides to his satisfaction that the test is a measure of innate capacity, he proceeds to use the test for every problem that for him is rationally dependent on that capacity. The test is used to assess mental status; inferences are then drawn concerning behavior on the basis of logical relationships between intelligence and behavior. It seems at times that so much research energy goes into the peripheral nature-nurture question that central questions for test utilization concerning test scores in relation to behavior are neglected. Once these questions are answered, it will be more reasonable to theorize concerning the nature of the intervening variables.

When an investigator asks if the intelligence of children is really affected by bilingualism, we have an example of an experimentally unanswerable question. This old problem was revived during the year (42), groups of bilinguals and monoglots being compared on a verbal test directly and with performance test achievement held constant. The difference in verbal scores persists in the analysis of covariance involving the performance test scores, but any result would be uninterpretable in terms of the question asked. The more useful questions to be asked here, to which answers can be obtained, are discussed in more general terms below.

Stability questions.—The stability of trait measures constitutes an area that is open to direct experimental attack. Stability can be measured in terms of gains or losses in mean score. It can also be measured in terms of rank-order of individuals in a group. Investigations proceed in the traditional experimental fashion with either of these measures being treated as a function of the possible conditions promoting instability, i.e., social class changes, emigration, bilingualism, nursery school attendance, age at which the environmental change is made, time between test and retest, and special training methods. It is conceivable that we can one day conclude that a stimulating nursery school will produce a mean gain of approximately 5 intelligence quotient (IQ) points on the Stanford-Binet, or that the test-retest correlations between 6 and 12 years of age on a given test for children of migratory workers is about .10 smaller than a similar correlation on the same test for children living in a more stable environment. It is inconceivable, however, that this research will lead to a conclusion concerning the percentage contribution of environmental factors to intelligence score variance.

Most of our information about stability of individual differences is restricted to intelligence tests. Reports on other measures have been few in number in the past, but their number is increasing. Strong (63) furnished significant information concerning the stability of interest scores. His results

show that stability is a function of age at initial testing, ranging in his study from high school through college, and of interval between testings, ranging up to 22 years. For the older age groups, interest scores are just as stable as intelligence test scores.

Doppelt & Bennett (19) reported on a three-year follow-up of scores on the Differential Aptitude Tests. The individual tests seem to have significantly different degrees of stability. Some of the correlations, in particular that for verbal reasoning, are comparable to those obtained for intelligence tests. Lennon's data (45) concerning stability of measures of achievement during the grade-school period are of interest. Specific achievement measures do not seem to be as stable as intelligence test scores, but within the group of achievement measures used, there are interesting differences in stability and in the age at which the final degree of stability becomes established. Reading is more stable than most, and this degree of stability is apparently established by the end of the second grade.

Test-retest data on the Graduate Record Examination were obtained by Heston (37). Certain of the achievement measures are almost as stable as the verbal factor score, but several are substantially lower. The general education index, a summation of achievement scores, is more stable than the measure of the verbal factor, although this is probably a function of the differential reliability of the two.

Studies of the stability of score levels as a function of experience were also reported during the year. Schultz (61) investigated the relationship between scores on the College Board mathematics tests as a function of amount of training in mathematics and the time interval since the last mathematics course. Similar studies (62) were reported for the achievement section of the Medical College Admission test. Small differences were obtained in the first of these studies for both variables. There were no significant differences in the latter study in which the less well-trained group still had the minimum number of hours required in the various areas.

Validity questions.—Predictive validities of tests can be obtained without reference to the nature-nurture question. If a test is administered experimentally at the time when a given decision would normally be made, i.e., to hire, to accept in college or in military service, to start training, or to guide into a vocation, and later is correlated with measures of proficiency, job satisfaction, or adjustment, substantial correlations cannot be cast aside because someone believes that the test measures past opportunities rather than native aptitude. Thus, increasingly, we are seeing introduced into aptitude test batteries measures of functions, such as mechanical information (18, 29) or premedical achievement (11), that would have been frowned on a few years ago.

The publishers of the Differential Aptitude Tests (18) are accumulating and disseminating many predictive validities of their tests for separate course grades. Although not generally available as yet, the Air Force (16, 28) has accumulated masses of data of this type for many technical training

criteria. These data are much more important to the test user than discussions about the psychological functions or factors measured by different tests. They are also basic data for the test theorist as well.

Information concerning the predictive validities of tests can also be integrated with the research on trait stability. The same factors that produce instability in the rank-ordering of individuals on tests should also produce changes in validity coefficients. Worbois (79) obtained validity coefficients on employment tests for criteria obtained 20 years after testing. Predictions were still substantial. Factors that produce gains or losses in mean score may also affect criterion performance to some degree. These latter studies are facilitated if objective criteria are used but are possible with adequate controls if ratings only are available.

Questions concerning group differences.—Disregard of the nature-nurture problem in investigations of race, sex, and nationality differences is also indicated. Again, a number of questions both useful and answerable can be asked. One of the simplest of these has to do with the extent of tested differences for various traits. Differences between groups on intelligence tests have been rather thoroughly explored, but the psychological and social significance of obtained differences in dealing with individuals has frequently been overlooked. That is, if the point biserial between groups and total score is .20, the variable is not a very important one no matter what the statistical significance of the mean difference may be. A correlation of this magnitude is very unlikely to produce a substantial beta weight in multiple regression equations. Group differences on trait measures other than intelligence have not been as thoroughly investigated. Primary mental abilities batteries should furnish very useful information here. Sex differences are of course known for the batteries in use since norms are furnished for the sexes separately (18, 55).

Stability of obtained group differences under various conditions such as those already listed for investigations of individual stability are also experimentally feasible. We can consider, for example, the increase in IQ's obtained for Negro children whose families have migrated from the southern to the northern United States as an objective and useful finding without worrying about whether it completely explains the lower test scores of the Negro group as a whole.

The predictive validities of various measures for special race, nationality or sex groups can also be investigated. In these validation studies, it is important not only to determine whether standard errors of measurement are equal, but also whether the same score has the same meaning in the groups being compared, i.e., whether the regression lines are identical or merely parallel. Gulliksen & Wilks (32) have published appropriate tests for these various comparisons.

An intellectually impoverished environment extending over the first 21 years of a Negro boy's life may well affect his intelligence test performance and in a relatively permanent fashion. It is not illogical to expect that a

pertinent criterion performance might be affected as well. The boy in question may, thus, not be a good bet for admission to a medical school if his admission test score falls below the level of other students accepted. When hypotheses such as these have been tested, it will be possible to use tests more intelligently than they are being used at present. Parenthetically, it is not inconsistent to desire these data and at the same time to work for social changes that will produce equality of opportunity. One study was noted of group differences, a comparison of British and Gurkha recruits (74), in which the mean differences were very substantial indeed. The author was able to hold to his original purpose, which was to validate the tests for military classification of the Gurkhas, in spite of strong temptation to succumb to the racial differences problem.

Parent-child resemblances.—Roff (58) has published a useful survey of information concerning parent-child resemblances for a broad list of traits. The range of correlations obtained between parents and children for the various traits reported is perhaps the most striking aspect of the data. These correlations range from near-zero values for certain personality measures to approximately .90 for political party preference. The correlation for measures of intelligence falls in an intermediate position, represented by a value of approximately .50. It is very difficult to assay all of the possible causal factors entering into these relationships, though it is obvious that both heredity and environment are involved and that both produce differences as well as similarities.

Several important conclusions can be drawn from these correlations without attempting an analysis of cause and effect in nature-nurture terms. Thus, the expectation of counselors that a son will usually be interested in his father's occupation is not borne out by the relatively low interest test correlations. On the basis of both interest and intelligence differences, we can conclude that the present degree of occupational similarity of father and son is probably too high for maximum personal satisfaction or for maximum social productivity. Regression from able parents to their offspring or from able offspring to their parents occurs in marked degree for most traits measured. Children having sufficient aptitude for college work, for example, will be found in all levels of our population, and many children of college graduates will fail to meet expectations of their parents or counselors. Selection of medical, law, or military school students cannot depend on parental occupation if efficient operation of our society is desired.

Trait Identification

The notion of innate capacity, or temperamental trend, is ruled out of any definition of trait, since these concepts do not result in measurement operations. It is important to establish other criteria that are subject to measurement. In this section, such criteria will be discussed, and the definition of trait will grow out of this discussion.

Homogeneity.—There is increasing emphasis upon a criterion for trait

measurement that is usually termed homogeneity. Homogeneous tests are considered necessary though not sufficient for this purpose. There is no general agreement as yet concerning the methodology for obtaining homogeneous tests, which in turn is related to the question of degree of homogeneity desired.

The items of a homogeneous test should measure only one common factor. Measurement of two or more factors makes the test a heterogeneous one. Because factor analysis is not a practical technique to apply to large numbers of test items, some substitute for factor analysis is needed for determination of homogeneity. Item analysis procedures involving correlations between items and total score are used to increase homogeneity, but are not completely satisfactory. In particular there are no criteria for deciding how high item-test correlations should be in order to have a satisfactory degree of test homogeneity. Bedell (3) published a criterion for deciding when to stop accepting items in order to obtain maximum reliability, which is a related problem.

Guttman (33) and Loevinger (47) suggested similar measures of homogeneity and criteria of item selection that are probably too severe, considering feasible degrees of reliability and generality to be obtained with psychological items. The criteria lead to very low values of homogeneity for typical tests. A high degree of homogeneity can be obtained only by the production of highly specific tests and multiplication of highly correlated traits. An outcome such as the latter may be useful for attitude measurement, but not for most other types of psychological measurement.

A technique developed by Loevinger, Gleser & DuBois (48) shows more promise for the construction of homogeneous tests. A prominent general factor should result from application of this method. The resulting tests should also be sufficiently broadly based that the number of trait measures will not be indefinitely multiplied. Wherry, Campbell & Perloff (77) reported the application of a technique described earlier by Wherry & Gaylord (78) that could also be used for the same purpose.

Independence of traits.—A trait measure should not be merely composed of reasonably homogeneous items, but the total score should have zero or near-zero correlations with measures of other traits. If each item in a test measures several functions, and these to equal degree, the test can be homogeneous without being a measure of a single trait.

Zero correlation among all trait measures represents the ideal. In ability measurement particularly, this seems to be an unrealizable goal. It is seemingly necessary to accept correlations greater than zero among various trait measures. The acceptable degree of correlation between traits is indicated by the following criterion:[2] the multiple correlation between any n-1 measures and the nth should be significantly less than unity when all correlations are corrected for attenuation. Additional trait measures can be added to the

[2] This criterion was first suggested to the writer by R. L. Thorndike and A. P. Horst during World War II.

group as long as the multiple correlation stays sufficiently low. Two measures should be substituted for one if this can be done without raising the multiple. Experimental independence of trait measures is obviously necessary; new traits cannot be invented by manipulation of basic data in several different ways as Krathwohl (43) did with his index of industriousness.

Stability and importance.—Stability and validity are necessary in any measuring instrument before it can be used intelligently. Research of this type was discussed in an earlier section. These same data are necessary also for the identification of traits. If a tentative trait measure shows no stability in a stable environment, it is doubtful if it should be accepted as a meaningful psychological trait. In the same way, if a tentative trait measure is not valid for any meaningful criterion, its psychological validity is questioned. Since to establish zero validity for everything is like proving the null hypothesis, one would still keep tests of unknown validity in a battery of trait measures for experimental purposes. Once validity is established, and in particular if differential validity for different criteria is established, the meaningfulness of the trait is assured and considerable information is available concerning its possible identification.

The reader may consider factor analysis to be the technique par excellence for the isolation and identification of traits. It is important, however, to observe that factor analysis is not an essential part of the process. If the measure has a suitable degree of homogeneity, independence, stability, and validity, all of which can be objectively determined, the measure can be considered to reflect a meaningful psychological trait.

Varieties of traits.—Traditionally, psychologists have talked about several different kinds of traits: aptitudes, interests, and personality traits. There do not seem to be any objective techniques, however, for trait classification, and apparently the rational classification is breaking down. Many might claim that the test of mechanical comprehension in the Psychological Corporation's Differential Aptitude Test series is in part a measure of interest, but it appears in an aptitude battery. The Air Force uses information tests and biographical data blanks in its aptitude test batteries (16). The Engineering Aptitude Test (20) handled by the Educational Testing Service weights heavily a technical vocabulary section. It may be more meaningful to classify tests in terms of their uses. In the above examples, the various tests were validated against proficiency criteria, thus making aptitude applicable. Other tests, or the same tests in different batteries, are validated against job satisfaction criteria. In this connection interest can be meaningfully applied.

Factor Analysis

If the number of tests to be analyzed were sufficiently small and if these tests were sufficiently simple functionally, there would be no need to use any factor analysis technique. Factor analysis is resorted to because of the complexities of present tests with the resulting complexities of their inter-

relationships. This problem becomes particularly acute when many tests are involved. Factor analysis is a procedure that may reduce the amount of labor involved in trait identification. It may also at times lead to ideas for more efficient measurement of traits.

One approach has almost pre-empted the field in the United States. Most investigators use estimates of communalities in the diagonals and rotate the mathematically obtained factors to meaningful positions. British factor analysts, on the other hand, make considerable use of the bifactor method, which produces a general factor plus group bipolar factors (12). There is also interest in the principal components solution which analyzes test variances and covariances (2, 36).

Rotated factors.—If an aid for trait identification is required, particularly if the traits are to be independent of a particular test battery, the use of communalities and rotation of factors is recommended. The user of this method should realize, however, that only hypotheses come from his factor analyses. When used in this fashion, factor analysis is only a partially objective technique. The decision as to when to stop factoring is perhaps the most subjective aspect of the procedure, though rotations also involve obvious subjective elements. As a result, the factor analysis does not lead to factual discoveries; it leads only to theories. To name and to describe psychologically the rotated factor loadings, which are one's theories concerning the traits involved, are useful only in so far as one's thinking about trait measurement and identification is helped. At times, however, the name permits closure, with the result that thinking is inhibited.

Up to the present, most factor analysis hypotheses have been checked by doing another factor analysis. Repetition of findings, or prediction of the direction of new findings, seems significant, and factor analysts gain confidence in their technique as a result. While this confidence is not completely misplaced, particularly when similar factors occur in many separate analyses, it is difficult to quantify the basis for this confidence; it is also difficult to quantify the degree of similarity in two factors necessary in order to conclude that they are identical. Repetition of findings in successive analyses with overlapping variables is possible only when there is a continuous research program. The publications from Thurstone's laboratory (17, 41, 68) represent this ideal. Reports of Air Force research (23, 29, 44) also illustrate this type of validation of findings.

If factor measures can be obtained, it is possible to test many factor analysis hypotheses by experimental designs involving differences between correlation coefficients. Thus, if a given change in test A should produce a certain change in factor loadings or pattern, these changes can be translated into predicted changes in correlations with factor measures B and C. Actual changes in correlations obtained can be evaluated by traditional tests of significance as the logic of experimental methodology demands. In a similar fashion, if n factors are theoretically present in a particular battery, then the n multiple correlations possible between one factor measure and the n-1

other factor measures should be less than unity after corrections for attenuation have been made. It must be recognized that obtaining the expected outcomes in this research does not validate the theory, but this is true of tests of theory of any sort. Negative results, on the other hand, should lead to theory changes.

Eysenck's study (21) can be classified as an example of this approach to the checking of hypotheses. His conclusions were based on analyses of differences between correlation coefficients, although he also completed a factor analysis of the same data. A similar approach might have been used by Michael, Zimmerman & Guilford (51) with good effect. Their factor analysis suggests that the hypothesis of a difference between visualization and spatial measures is confirmed, but one would be happier if appropriate critical ratios were provided.

Orthogonal versus oblique rotations.—Oblique rotational solutions are seemingly becoming more popular than orthogonal solutions, but the issues involved here have not been clearly stated. A preference for orthogonal solutions does not involve closing one's eyes to correlated factors or refusal to recognize general factors underlying the group factors. These facts are obvious from inspection of the factor loadings in most tables of rotated factor loadings in which orthogonality is preserved. Oblique factors, on the other hand, do not describe more accurately the nature of reality since the factors are simply concepts used to describe the intercorrelations of a group of tests, arbitrarily constructed and arbitrarily selected. Authors preferring oblique solutions stress the greater accuracy of fit for the data at hand, even though both a table of factor loadings and a table of factor intercorrelations are necessary to describe the original correlational matrix. The present writer is generally better satisfied with the orthogonal frame of reference, involving fewer descriptive statistics, even though the fit in any one study is less accurate.

Degan (17) repeated an analysis of Air Force data which had originally been published in orthogonal form (29). The same factors are defined by the same tests in his oblique solution as in the orthogonal solution. He gives different names to certain factors, but these are the result of introspective differences. Jones (41) repeated his own previously published orthogonal analysis of Stanford-Binet items at age 13. Similarities between solutions are still marked, but results are not identical since more factors were extracted and rotated in the second analysis. Fruchter (23) reported both types of solutions for some postwar Air Force data, and with number of factors constant in the two analyses, the results are highly comparable.

It is necessary to hold not only number of factors constant in these comparisons, but also the theory reflected by the rotations. Sutherland (64) compared orthogonal and oblique solutions and found that all factors but one were comparable. One factor in the orthogonal solution had been considered a general factor so that it was allowed to account for much of the variance. If this variance had been distributed among the other group factors, allowing

the general factor to be indicated by the lack of zero loadings, the results would have been more comparable to those obtained in the oblique solution.

Second-order factors.—If only one general factor were necessary to explain the intercorrelations of oblique ability factors, the writer doubts whether departures from orthogonality would have any interest. In this case the consequences for trait measurement and test construction would be severely limited. The general factor could not be measured except by tests of the sort used to measure the group factors. Thus, the construction of a measure of "g" would be superfluous since all necessary information could be obtained from the separate trait tests. Research concerning the origin of "g" presumably involves one immediately in the dead-end of the nature-nurture question, a consequence of little experimental interest.

The hypothesis that more than one second-order factor is necessary to explain the intercorrelations of oblique ability factors is supported by many studies in which second-order factors have been extracted. Rimoldi (56), for example, has more than one second-order factor on tests all supposed to be measuring "g." This hypothesis is more productive of research possibilities, though attempts to date to identify these second-order factors have been extremely tentative. Do second-order factors reveal certain developmental sequences, patterns of differential training, experience, past interests, or present motivation? Under what circumstances could a measure of a given second-order factor be substituted for the several group factor measures? To what extent do second-order factors depend on the accidents of test construction and selection? It is believed that these are important problems capable of solution.

Other applications.—Two relatively new applications of factor analysis will attract more attention in the next few years than they have this past year. The analysis of qualitative data is one of these, with Burt (12) reporting one contribution during the year. The other is inverse analysis, or Q-technique, of which Holley & Buxton (38) have one example. The latter is particularly appropriate in clinical research where the investigator typically has or can get a great deal of data on each subject, but where the number of subjects is often very limited.

Experimental methods.—In the very early stages of trait analysis, one is frequently faced with the problem that no tests of any sort are available in the area to be investigated. Furthermore, the test ideas available are sufficiently complex and time consuming that to use all in a factor analysis would be extremely tedious. It is frequently possible under these circumstances to select extreme criterion groups and to obtain differences between the means of these groups for a variety of situations that presumably involve the hypothetical trait.

Block (7) was interested in the possible trait of ego control. Criterion groups were selected by intensive clinical interviewing techniques. Subjects were presented with a wide variety of situations having in common only the possibility that a trait of ego control would affect results. Breadth of test

XUM

situations is crucial if the investigator wishes to draw conclusions concerning the generality of the trait. Block found that his results were generally in line with expectations in all situations, thus providing the evidence for generality. Differences were not large, on the other hand, indicating that the trait did not contribute prominently to the variance in any one of the experiments. It is possible that personality trait measures will generally show this phenomenon. It has been pointed out[3] that we may have to treat these separate situations much as items in an aptitude test are treated. Individual items are not very reliable and alone contribute little to the final homogeneous score, the latter being the result of pooling of many items.

Traits Versus Types

There is considerable interest in the Sheldon types. This interest illustrates the attractiveness that type concepts have generally rather than the quality of the research responsible for the ones presently in the limelight. Types have the advantage that they are easy to think about, are usually few in number, and also explain so much. The trait is generally more useful, but does not have the above advantages.

Criticisms of Sheldon's types.—Sheldon's physical types originated in the arm chair. The three temperamental types resulted from his statistical criteria of trait-rating selection, the criteria leading inevitably to no more and no less than three types. The intercorrelations of the type scores for either physique or temperament indicate that the potentially valid variance in any one type is perfectly predicted from the other two. Only two types need be used for research purposes; only two types should be used in factor analyses or multiple correlation problems.

Types are defined by measures that are relative to other measures within the individual. The whole person is always completely described by the three type scores without regard, for example, to absolute size or strength. Any expectation of high correlations with proficiency measures, trait measures, or any other scale which ranks individuals without regard to intra-individual relationships is, therefore, quite unrealistic. It is desirable to correlate type scores with other type scores. This Sheldon did in obtaining his relationships between physique and temperament, but he made another mistake at this point in allowing himself to make both sets of ratings.

In the validation of type scores, criteria should be sought that do not involve an ordering of individuals from high to low on some continuum. Delinquency may be a criterion of this type. The careful study of Glueck & Glueck (27) in which a nonchance relationship was found between physical type and their delinquency-nondelinquency criterion appears to be the best support than can be mustered for any validity of the Sheldon types.

Substitutes for typing.—In obtaining relationships with physique, it is probable that a more empirical approach would yield greater validity. If

[3] This point was made by Lee Cronbach at an Air Force conference on the objective measurement of personality.

the Gluecks (27) had applied the discriminant function to a series of physical measurements, even greater separation of their criterion groups would probably have been obtained. Research workers interested in problems of physique are advised to discard the Sheldon types, to obtain a set of trait measures as here defined, and to use these trait measures in combination by applying well-recognized statistical techniques.

Test Construction

Two important volumes of general interest in this area were published during the year. Fortunately, they supplement each other more than they overlap. They should become basic reference works for every test constructor. The statistics of test theory is presented compactly and precisely by Gulliksen (30). This work constitutes an extension of the earlier discussion by Thurstone (67) which has not been available for some time. There are certain deficiencies—one finds no discussion of the shortcut methods of item analysis that involve dichotomizing the test score distribution, for example—but any tendency toward a too puristic approach is compensated for by the other volume. Lindquist and his collaborators (46) produced a book that is nothing if not practical. Chapters are included, for example, on the preparation of printer's copy and administration of a large-scale testing program. Other chapters range from the philosophy of testing in the schools, through test construction techniques, to the logic of reliability, validity, standardization, and use of tests in batteries.

More specific journal contributions are categorized under various headings and are discussed briefly below. In almost every case, one could also discuss the contributions these volumes make to the same problems, but this is not done because of space limitations.

Test rationales.—As a starting point for test construction, the critical incident technique of job analysis still flourishes. Wagner (73) discusses this approach with reference to tests for dentists, for example. There seems little doubt that this method shows more promise for the purpose than other approaches to job analysis. It should be emphasized, however, that job analysis is just the starting point for test construction. In the past, other forms of job analysis have been perverted to serve as a substitute for empirical validation and weighting procedures. This application is still hazardous, even with newer techniques.

Flanagan (22) calls attention to the use of comprehensive item rationales in test construction. Travers (70) also supports this approach and criticizes blind empiricism. It is useful to emphasize that there are still several places in test construction where a psychologist should think as well as apply his statistical techniques. This area seems to be one of the most appropriate for taking thought.

Item analysis.—Even though there is perhaps more interest today in the construction of homogeneous tests, for which appropriate techniques were mentioned earlier, there is still occasional need for the development of a high-

XUM

ly valid test. Gleser & DuBois (26) present a new substitute for the multiple regression solution. Substitutes are, of course, necessary because of the amount of work involved in the more precise handling of large numbers of test items. Mollenkopf (53) has published a study of the effect on typical item statistics for items in the latter part of a speeded test. He concludes that misleading information is obtained if, roughly, fewer than 50 per cent of the sample finish the test.

Scoring formulas.—The traditional scoring formula which supposedly compensates for chance success is criticized by Hamilton (34). The proposed substitute makes the same psychologically meaningless assumptions that have always been made concerning guessing. Lyerly (50) suggests that a more empirical approach, which would maximize validity, is more useful. This does not necessarily solve the problem of the use of tests in batteries, even for the prediction of a single criterion, since it is battery validity that should be maximized. If several criteria are to be predicted by the battery, it may be more useful for certain speeded tests with adequate reliability and independence of the error scores to make use of both the number correct and the number of errors in weighting of variables.

Reliability.—Test reliability is not receiving much attention. Since validity data are so much more important, this lack of emphasis is not unfortunate if research time is spent on more important problems. Gulliksen (31) presents a useful method of estimating the reliability of a somewhat speeded test from split-half and item analysis data. Johnson (40) discusses the problem of which type of reliability coefficient to use in corrections for attenuation and concludes that the test-retest method is preferable. Comparable form and split-half coefficients are discarded since specific factors may be involved and may produce an overcorrection. Memory factors in the test-retest situation, on the other hand, may produce an undercorrection. Perhaps the solution is for test analysts to make use of reliability coefficients determined in several different ways and with several different time intervals between test and retest or between comparable forms. The one perfect method of obtaining a reliability estimate does not exist.

Validity.—Brogden & Taylor (9) presented a logical analysis of the criterion. They classify criterion errors and emphasize the importance of independence of test and criterion. Other types of errors are important but do not destroy the usefulness of validation data, since they do not lead to spurious results. Contamination of criterion with test information, however, is at all times to be avoided. Furst (24) also has a theoretical discussion of the criterion problem with regard to the selection of students for professional schools.

Search for an ultimate criterion is not necessarily indicated. Since criteria err in several different ways, it is more useful to validate tests against multiple criteria of the same thing or to analyze a criterion into its several components and validate against the part scores. Ghiselli & Brown (25) did the former in the validation of a battery of mechanical tests, utilizing both tech-

nical-school course grades and a later criterion on the job. The Psychological Corporation does the latter in its validation program for the Differential Aptitude Tests, utilizing single course grades rather than over-all grade-point average.

Taylor (65) has an alternate derivation to Horst's solution to the problem of maximizing battery validity for a fixed total of testing time. This approach should be useful for a selection battery, but is not applicable directly when multiple criteria are involved. In the latter case, as Brokaw (10) shows, it is usually possible to shorten the lengths of all tests without appreciable effect on battery validity. It should be noted that shorter tests are not indicated when individual tests are to be interpreted or when test differences are to be interpreted.

Cross-validation.—An important symposium concerning the problem of cross-validation was published during the year (54). Mosier's discussion clarifies problems of terminology. Thus, Hay's study (35) would be called one in "validity extension" rather than one involving cross-validation. Cureton and Katzell (54) make clear the importance of cross-validation of test and item weights. Cureton also reinforces this argument in another article (13). Although both the concept and the need are becoming better known, it is not difficult to find examples of studies that would have benefited from this relatively simple technique. Belman & Evans (5) selected only six variables from 26 for use in a selection battery. Smallness of sample is no argument against the use of the technique, particularly if double cross-validation is used. Glueck & Glueck (27) could have made a more substantial contribution to knowledge of delinquency with little additional effort if they had split their sample and cross-validated their measures.

One event of the year demonstrated rather clearly that cross-validation is becoming generally recognized as desirable, even if operations that are both unique and meaningless are invented to satisfy the need. The authors of a new aptitude series (57) claimed that their tests were cross-validated when they demonstrated that the relationship between percentile and standard scores for one test was the same as for a criterion test.

Wherry (54) adds a final note of caution on the symposium concerning dependence on empiricism alone. This argument takes two forms. One criticism concerns the unthinking acceptance of any obtainable criterion against which to cross-validate. The other is related to the fact that, in addition to the shrinkage expected in a second sample from capitalizing on chance in the first, one also expects the presence of the usual sampling errors that can either increase or decrease coefficients; i.e., a shrunken measure might show either more or less validity in the second sample. Strict empiricism, again, is not a cure-all.

Correction for range of talent.—Votaw, Rafferty & Deemer (72) present a solution to the estimation of parameters in a truncated trivariate normal distribution. This constitutes an interesting extension of the bivariate case, but does not seem to add anything of practical importance for the test con-

structor. The problem of correcting for restriction of range of talent when the criterion is dichotomous is not met by traditional formulas. Birnbaum (6) published a procedure for estimating elimination rate when the cutting score on the test is lowered that is related to the restriction of range problem when using biserials.

Test Utilization

The use of tests in batteries is receiving increasing attention by test technicians, just as test batteries are becoming increasingly popular with test users. The older aptitude tests were generally designed to predict specific criteria, e.g., engineering proficiency, and were typically complex functionally and time consuming. This type of test is still adequate for selection purposes, but for either guidance or personnel classification, the number of tests of this type and the time consumed in their administration makes their use unwieldy. Logically, following this procedure, one needs a separate aptitude test for each criterion. Present trends, which started for all practical purposes with the publication of Thurstone's Primary Mental Abilities (55) and received considerable impetus from wartime classification experience, are toward batteries of tests measuring various psychological functions that are common to many criteria. Of the several ways of combining these tests, the standard procedure is that of multiple regression, though clinical intuition is undoubtedly used more commonly.

Multiple regression.—The most comprehensive discussion of the use of tests in personnel classification was published by Thorndike (66). Much of his contribution can be applied to guidance problems as well. The case for a test battery of separate measures of unique psychological functions is clearly presented. The combination of these measures by the use of multiple regression procedures to obtain maximum predictive efficiency is recommended. A solution to the problem of differential prediction of two criteria, derived by Mollenkopf (52), is discussed. The unsolved problems of differential prediction of more than two criteria are made explicit and some approximate procedures are suggested for use.

Although classification is most efficient if the tests are independent and have zero validities for at least certain criteria, Brogden (8) shows that these optimum conditions do not have to be very closely approximated for differential prediction to be extremely useful. The problem of classification can be considered one of multiple and simultaneous selection. When the number of jobs is quite large, the selection ratio for any one is so favorable that small increments of differential validity lead to appreciable gains in efficiency. With perfectly reliable measures, intercorrelations could increase to .80 with loss of only one-half of the gain to be obtained from optimum differential prediction.

Wesman & Bennett (76) also discuss problems of differential prediction, pointing up some of the difficulties and the conditions under which differential prediction is possible. They conclude that counselors need both ab-

solute and relative probability values for future criterion scores. Multiple regression techniques give the former. Subjective profile interpretations have typically been used for the latter. A new statistical technique gives promise, however, of providing more valid information of this type.

Multiple discriminant function.—The best introduction to the multiple discriminant function is to be found in the symposium by Tiedeman, Rulon & Bryan (69). The first of these authors discusses the history of the discriminant function. The second describes the situations in which it is suitable and distinguishes between multiple regression and the multiple discriminant function. Rulon also discusses this problem elsewhere (59) in a less detailed, but also less formal, fashion. The third member of the symposium furnishes the mathematical basis. Anderson (1) also has a contribution of the latter type.

The fact that the results from the two techniques differ has been obscured, as Rulon points out, by the fact that the simple discriminant function when applied to pass-fail data for a single criterion gives identical information to that furnished by multiple regression. This, however, is an isolated case. If successful pilots are contrasted with successful navigators, even the simple discriminant function gives quite different information from that furnished by multiple regression applied to each of the criteria separately. In a pure classification situation, the former is more useful. A man may be only a mediocre pilot candidate, as revealed by the multiple regression technique, but he may be more like a successful pilot than like a successful navigator. In other words, the multiple discriminant leads to empirical types. If a population has to be classified, valid typing is useful. It is obvious that a discriminant function cannot be validated in the way that the aggregate score obtained by multiple regression procedures can be. Correlations with proficiency criteria are meaningful only for the latter. Forced choice tests also throw people into categories in much the same way as does the discriminant function. It may be equally useless to validate these tests in traditional ways. Wesley, Corey & Stewart (75) demonstrate that correlations between Kuder Preference Record scores and aptitude scores are higher if aptitude scores are assigned to individuals on the basis of their own aptitude levels. If typical trait scores, in other words, are converted to ability types, correlations with interest types are raised. In the same way, one would expect Kuder scores to be more highly correlated with relative job satisfaction for the individual than with level of job satisfaction relative to other individuals.

Simple discriminant function.—Horst & Smith (39) published a use of the discriminant function that might be termed classical if the technique were a little older. It was originated in order to handle problems such as species differentiation; the Horst & Smith contribution demonstrates the separation of racial groups. Similarly, one could distinguish delinquents and nondelinquents, neurotics and normals, or men and women for any set of measurements. The paper by Schmid (60) makes a minor computational contribution to the use of the technique. Lubin's discussion (49) is useful in relating the discriminant function to better known statistical techniques.

Other methods of combining scores.—The writer discovered two comparisons of multiple regression and multiple cut-off procedures. In one (79), the former generally gave higher validity; in the second, the latter (35). The number of cases was sufficiently small that the significance of either is doubtful. In the second case, there was no cross-validation.

Tucker (71) compared multiple regression with a profile procedure deriving from Toops. His results indicate that in certain circumstances the profile procedure is almost as valid as multiple regression, but is more cumbersome. Bell (4) added two additional profile methods to that used by Tucker, compared all three with multiple regression, and checked in addition to see if any added to the validity obtained from multiple regression. Profile validities are consistently lower and add nothing to the validity obtainable by the traditional technique.

Many psychologists object to the simple additive assumptions made both by the multiple regression procedure and by the multiple discriminant function. Intuitive interpretations, on the other hand, are under increasing fire. This will undoubtedly lead to invention and try-out of several more objective methods of profile interpretation, as well as to further exploration with present ones. In the meantime, the present author is willing to place a small bet on the outcome of the following comparison: the Rorschach test to be administered according to present procedures and interpretations correlated with some later, objective criterion, e.g., delinquency; a series of short inkblot tests, each designed to measure adequately a function covered by the present test, to be administered, validated, weighted, and cross-validated for the same criterion; over-all validation of the two procedures to decide the issue.

LITERATURE CITED

1. Anderson, T. W., *Psychometrika*, **16**, 31–50 (1951)
2. Bartlett, M. S., *Brit. J. Psychol., Statist. Sect.*, **3**, 77–85 (1950)
3. Bedell, B. J., *Psychometrika*, **15**, 419–30 (1950)
4. Bell, R. Q., *A Comparison of Profile Prediction Methods and Multiple Correlation* (Doctoral thesis, Stanford Univ., Stanford, Calif., 152 pp., 1951)
5. Belman, H. S., and Evans, R. N., *J. Educ. Psychol.*, **42**, 52–58 (1951)
6. Birnbaum, Z. W., *Psychometrika*, **15**, 385–89 (1950)
7. Block, J., *An Experimental Investigation of the Construct of Ego-Control* (Doctoral thesis, Stanford Univ., Stanford, Calif., 124 pp., 1950)
8. Brogden, H. E., *Educ. Psychol. Measurement*, **11**, 173–95 (1951)
9. Brogden, H. E., and Taylor, E. K., *Educ. Psychol. Measurement*, **10**, 159–86 (1950)
10. Brokaw, L. D., "The Comparative Composite Validities of Batteries of 'Short' vs. 'Long' Tests," *Research Bull., Air Training Command, Human Resources Research Center*, No. 50-1 (1950)
11. *Bull. of Information, 1950, Med. College Admission Test* (Educ. Testing Service, Princeton, N. J., 1950)
12. Burt, C., *Brit. J. Psychol., Statist. Sect.*, **3**, 166–85 (1950)
13. Cureton, E. E., *Educ. Psychol. Measurement*, **10**, 94–96 (1950)
14. Davis, A., *Phi Delta Kappan*, **32**, 253–56 (1951)
15. Davis, A., *School Life*, **33**, 87 (1951)
16. Dailey, J. T., and Gragg, D. B., "Postwar Research on the Classification of Aircrew," *Research Bull., Air Training Command, Human Resources Research Center*, No. 49-2 (1949)
17. Degan, J. W., "A Re-analysis of the Army Air Force Battery of Mechanical Tests," *Rept. No. 58* (Psychometric Lab., Univ. of Chicago, Chicago, Ill., 1950)
18. *Differential Aptitude Tests Manual* (Psychol. Corp., New York, N. Y., 1948); Suppls., 1949, 1950, 1951
19. Doppelt, J. E., and Bennett, G. K., *Educ. Psychol. Measurement*, **11**, 228–37 (1951)
20. *Engineering Aptitude Test* (Educ. Testing Service, Princeton, N. J., 1951)
21. Eysenck, H. J., *Brit. J. Psychol*, **42**, 114–22 (1951)
22. Flanagan, J. C., *Educ. Psychol. Measurement*, **12**, 151–55 (1951)
23. Fruchter, B., "The Factorial Content of the Airman Classification Battery: I. Factor Analysis of 1948 Normative Survey Battery," *Research Bull., Air Training Command, Human Resources Research Center*, No. 49–1 (1949)
24. Furst, E. J., *Educ. Psychol. Measurement*, **10**, 637–53 (1950)
25. Ghiselli, E. E., and Brown, C. W., *J. Applied Psychol.*, **35**, 23–24 (1951)
26. Gleser, G. C., and DuBois, P. H., *Psychometrika*, **16**, 129–37 (1951)
27. Glueck, S., and Glueck, E., *Unravelling Juvenile Delinquency* (Commonwealth Fund, New York, N. Y., 399 pp., 1950)
28. Gragg, D. B., and Gordon, M. A., "Validity of the Airman Classification Battery AC-1," *Research Bull., Air Training Command, Human Resources Research Center*, No. 50-3 (1950)
29. Guilford, J. P., and Lacey, J. I., *Printed Classification Tests* (U. S. Govt. Printing Office, Washington, D. C., 919 pp., 1947)

30. Gulliksen, H., *Theory of Mental Tests* (John Wiley & Sons, Inc., New York, N. Y., 486 pp., 1950)
31. Gulliksen, H., *Psychometrika*, **15**, 259–69 (1950)
32. Gulliksen, H., and Wilks, S. S., *Psychometrika*, **15**, 91–114 (1950)
33. Guttman, L., *Am. Sociol. Rev.*, **9**, 139–50 (1944)
34. Hamilton, C. H., *Psychometrika*, **15**, 151–68 (1950)
35. Hay, E. N., *J. Applied Psychol.*, **34**, 153–58 (1950)
36. Henrysson, S., *Brit. J. Psychol., Statist. Sect.*, **3**, 159–61 (1950)
37. Heston, J. C., *Educ. Psychol. Measurement*, **10**, 367–70 (1950)
38. Holley, J. W., and Buxton, C. E., *Educ. Psychol. Measurement*, **10**, 400–10 (1950)
39. Horst, P., and Smith, S., *Psychometrika*, **15**, 271–89 (1950)
40. Johnson, H. G., *Psychometrika*, **15**, 92–119 (1950)
41. Jones, L. V., "Primary Mental Abilities in the Stanford-Binet, Age 13," *Rept. No. 71* (Psychometric Lab., Univ. of Chicago, Chicago, Ill., 1951)
42. Jones, W. R., and Stewart, W. A. C., *Brit. J. Psychol., Statist. Sect.*, **4**, 3–8 (1951)
43. Krathwohl, W. C., *J. Educ. Psychol.*, **42**, 97–100 (1951)
44. Lecznar, W. B., Fruchter, B., and Zachert, V., "A Factor Analysis of the Airman Biographical Inventory BE601B," *Research Bull., Air Training Command, Human Resources Research Center*, No. 51-3 (1951)
45. Lennon, R. T., *Educ. Psychol. Measurement*, **11**, 121–27 (1951)
46. Lindquist, E. F., Ed., *Educational Measurement* (Am. Council on Education, Washington, D. C., 819 pp., 1951)
47. Loevinger, J., *Psychol. Monographs*, **61**, 1–49 (1947)
48. Loevinger, J., Gleser, G. C., and DuBois, P. H., *Maximizing the Resolving Power of a Test* (Presented at Midwestern Psychol. Assoc. Meeting, Chicago, Ill., April 27–28, 1951)
49. Lubin, A., *Brit. J. Psychol., Statist. Sect.*, **3**, 90–103 (1950)
50. Lyerly, S. B., *Psychometrika*, **16**, 21–30 (1951)
51. Michael, W. B., Zimmerman, W. S., and Guilford, J. P., *Educ. Psychol. Measurement*, **10**, 187–213 (1950)
52. Mollenkopf, W. G., *Psychometrika*, **15**, 409–17 (1950)
53. Mollenkopf, W. G., *Psychometrika*, **15**, 291–315 (1950)
54. Mosier, C. I., Cureton, E. E., Katzell, R. A., and Wherry, R. J., *Educ. Psychol. Measurement*, **11**, 5–28 (1951)
55. *Primary Mental Abilities Tests*, Rev. Ed. (Science Research Associates, Chicago, Ill., 1947)
56. Rimoldi, H. J. A., *Psychometrika*, **16**, 75–99 (1951)
57. Roeder, W. S., and Graham, H. B., *Aptitude Tests for Occupations* (Calif. Test Bureau, Los Angeles, Calif., 1951)
58. Roff, M. F., *J. Psychol.*, **30**, 199–227 (1950)
59. Rulon, P. J., "The Stanine and the Separile: A Fable," *Educ. Research Corp. Bull.* (February, 1950)
60. Schmid, J., Jr., *Psychometrika*, **15**, 431–34 (1950)
61. Schultz, D. G., *Psychometrika*, **15**, 369–84 (1950)
62. Schultz, D. G., *Educ. Psychol. Measurement*, **12**, 138–50 (1951)
63. Strong, E. K., *J. Applied Psychol.*, **35**, 89–91 (1951)
64. Sutherland, J., *Brit. J. Psychol., Statist. Sect.*, **4**, 21–30 (1951)
65. Taylor, C. W., *Psychometrika*, **15**, 391–406 (1950)

66. Thorndike, R. L., *Psychometrika*, **15**, 215–35 (1950)
67. Thurstone, L. L., *The Reliability and Validity of Tests* (Edwards Brothers, Ann Arbor, Mich., 113 pp., 1931)
68. Thurstone, L. L., *Psychometrika*, **16**, 11–20 (1951)
69. Tiedeman, D. V., Rulon, P. J., and Bryan, J. G., *Harvard Educ. Rev.*, **21**, 71–95 (1951)
70. Travers, R. M. W., *Educ. Psychol. Measurement*, **11**, 128–37 (1951)
71. Tucker, J., *Relative Predictive Efficiency of Multiple Regression and Unique Pattern Techniques* (Doctoral thesis, Columbia Univ., New York, N. Y., 62 pp., 1950)
72. Votaw, D. F., Jr., Rafferty, J. A., and Deemer, W. L., *Psychometrika*, **15**, 339–47 (1950)
73. Wagner, R. F., *J. Applied Psychol.*, **34**, 190–92 (1950)
74. Warburtin, F. W., *Brit. J. Psychol.*, **42**, 114–22 (1951)
75. Wesley, S. M., Corey, D. Q., and Stewart, B. M., *J. Applied Psychol.*, **34**, 193–97 (1950)
76. Wesman, A. G., and Bennett, G. K., *Educ. Psychol. Measurement*, **11**, 265–72 (1951)
77. Wherry, R. J., Campbell, J. T., and Perloff, R., *Psychometrika*, **16**, 67–74 (1951)
78. Wherry, R. J., and Gaylord, R. H., *Psychometrika*, **8**, 247–64 (1943)
79. Worbois, G. M., *J. Applied Psychol.*, **35**, 15–19 (1951)

XUM

PERSONALITY[1]

By H. J. Eysenck

Psychology Department, University of London Institute of Psychiatry, London, England

Personality is probably the most general and the least well defined term in use in psychology, and it is not surprising that the selection of topics to be dealt with, and papers to be included, in this chapter is even more arbitrary than is the case in other parts of this book. Inevitably, a review of this kind will serve the function of a projective technique as much as that of an objective appraisal, and the firm belief that this procedure is not contrary to the aims of the editors encourages me to make no apologies for structuring this review around certain firmly held views regarding the nature and development of personality. These views are not altogether in line with much current thinking, and it may be worth while to mention them briefly so that the reader may be able to discount possible bias. In the first place, I regard the study of personality as a scientific discipline subject to all the customary dictates of scientific methodology. This, to my mind, excludes the clinical, idiopathic, and intuitive methods of approach, except as sources of hypotheses. Secondly, I would lay much greater stress on constitutional and hereditary factors in personality than is common; fortunately, there is evidence to be reported in this chapter which is very relevant to this problem. Thirdly, I believe in the importance of classificatory or taxonomic problems in the early stages of a science. Again, there is ample material in this review to illustrate this belief and put it into an experimental setting.

The discussion will be in four main parts. The first relates to personality structure; the second to personality development; the third to methodology and measurement, and the fourth to theory. Also included is a section on textbooks and summaries.

Organisation

Probably the most important event during the past year has been the appearance of the twentieth edition of Kretschmer's *Körperbau und Charakter* (56). It would probably be true to say that very few authors have been more misrepresented by writers of textbooks and even by research workers in their own field than has Kretschmer. The reasons for this are not far to seek. An English translation of Kretschmer's work appeared early in the 1920's and has served as a source book for most English and American psychologists ever since. While this edition sets out Kretschmer's main hypothesis regarding the correlation between schizothymia and leptosomatic body build and between cyclothymia and pyknic body build, it does not even

[1] The survey of the literature to which this review pertains was completed in May, 1951.

adumbrate the vast amount of experimental work in many fields which has since been carried out in Kretschmer's laboratories in Marburg and then in Tübingen. This material has been incorporated in successive editions of Kretschmer's book—a custom common on the European continent but quite alien to the Anglo-American tradition. [Fisher's *Statistical Methods for Research Workers* (27) is the one exception that proves the rule.] Hence, the subtle changes that have taken place in Kretschmer's way of thinking about the problem of types and the vast structure of psychological testing which he has built up on the basis of his theories have been almost completely neglected by workers in the field of personality.

Essentially, Kretschmer's system is a typology, but we should be careful to understand the word "type" in the sense which Kretschmer himself gives to it and avoid knocking down again the man-of-straw built up by textbook writers and used for generations to scare away the young student from this important area of psychological thinking. To Kretschmer, a type is not an either-or system of absolute classification; he defines it essentially in terms of observed correlations:

> Konstitutionsforschung ist Korrelationsforschung. . . . *Typenforschung*—dies wollen wir klar festhalten—*beginnt erst dort, wo empirische Zusammenhänge und Korrelationen zwischen biologischen Merkmalsgruppen nachgewiesen werden, die man vorher nicht kannte oder nicht beweisen konnte.*

While Kretschmer is not familiar with factor analysis, his methodology, as far as it is expressed in words, shows a striking resemblance to procedures advocated by factor analysts, and, indeed, his thinking can be translated almost directly by substituting the word "factor" for the word "type." For all those who take seriously the theoretical dilemma posed by the traditional discussion of type versus trait psychology, Kretschmer's chapter on "Der Konstitutionstypus als naturwissenschaftliches und erkenntnistheoretisches Problem" must surely be required reading.

When we turn from the general question of type to Kretschmer's experimental procedure, it is impossible not to be impressed by the ingenuity of his approach and the psychological acumen shown in his choice of concepts. In the absence of a translation of Kretschmer's chapters on "Beizehung zwishen Körperbau, Persönlichkeit und Psychose im Experiment" and "Konstitution und Leistung," the reader may wish to refer to a brief summary of some of these findings which recently appeared in the *Journal of Personality* (18). This summary also deals with the very ingenious experimental method which Kretschmer has used throughout to prove certain points important to his general theoretical system, such as, for instance, the essential similarity of nonpsychotic, normal individuals to the two groups which he regards as the prototypes of his typology, i.e. the manic-depressives and the schizophrenics. It will be noted that Kretschmer has anticipated a good deal of the current interest which is being taken in the relation between personality and perception and has also shown many important relations between personality and motor phenomena. Of great

XUM

interest, too, are his experimental studies on the relation between personality and autonomic functioning.

I do not wish to give the impression that Kretschmer's system can be regarded as definitely established. There are obvious weaknesses in his procedure although these do not necessarily coincide with those which appear obvious to most readers at first. The fact, for instance, that there is a complete absence of indices of statistical validity is not of any great importance; in going through 100 or so studies from Kretschmer's laboratory, I have routinely calculated these indices and found that, in nearly all cases, reported differences were significant at the 1 per cent level. It is easy to "make the dubious assumption that scientific wisdom increases by steps significant at the 5 per cent level" (7). There must be thousands of questionnaire studies, meticulously accurate in their statistical treatment, which, nevertheless, are completely barren of any psychological interest; Kretschmer's work is of the highest importance psychologically, and though I would be the last to advocate lack of rigorous statistical treatment, I feel that such treatment is only appropriate when it is used on worth-while data accumulated in an attempt to test a worth-while hypothesis.

A typology similar to Kretschmer's in some ways, but laying great importance on embryonic constitution, is that of Martiny (65), who writes in the same tradition as other Continental authors whose hypotheses preceded Sheldon's (89, 90, 91) and were taken up by him later. According to Martiny,

La biotypogenèse permet en s'appuyant sur l'embryogenèse, de considérer 4 biotypes genetiques, véritables constitutions de base, l'entoblastique, le mésoblastique, le chordoblastique, l'ectoblastique. . . . La constitution entoblastique, dans l'absolu et le relatif, se marque anatomiquement par sa rondeur tissulaire, par la grosseur plus grande des organes du tube digestif; physiologiquement par l'exagération fonctionnelle des glandes endocrines, appareils et systèmes de l'anabolisme anaérobique; psychologiquement par une prédisposition au statisme endesthésique.

La constitution mésoblastique ajoute l'épanouissement de ses organes, appareils et systèmes issus de son tissu primitif à ceux d'un entoblaste normalement développé. Le mésoblastique anatomiquement trapu et sanguin, physiologiquement anabolique aérobique, est psychiquement mû par un dynamisme amphi-esthésique.

La constitution chordoblastique jouit d'un parfait équilibre par l'apport à égalité des éléments de l'ectoblaste à ceux de l'entomésoblaste. Le chordoblastique anatomiquement élancé et musclé, physiologiquement catabolique aérobique, est psychiquement en possession d'une harmonie cénesthésique.

La constitution ectoblastique se traduit par une carence des éléments entomésoblastiques, laissant une prévalence absolue et relative au système neuro-sensoriel. L'ectoblastique anatomiquement mince et chétif, physiologiquement catabolique anaérobique, est psychiquement prédisposé à un détachement exesthésique.

As with Kretschmer, it is impossible to summarize the wealth of information given in this book and the many suggestive correlations posited between physiology, anatomy, psychology, psychiatry, and other disciplines. There is hardly any parallel in the Anglo-Saxon countries to the stress laid on interdisciplinary constitutional work on the Continent, and in view of the lack

of suitable translations and the abandonment of language requirements by many universities, it is unfortunately likely that this gap will become more and more marked. Perhaps a closer perusal of Continental journals, such as *Biotypologie* and of summary articles such as that of Schreider (86) will do something to bridge this gap.

Two British articles on body build (79, 80) provide the transition to a typological system less wide in its impact than the French or German and more strictly controlled from the statistical and experimental point of view, namely that expounded by the writer in *Dimensions of Personality*, a book whose appearance in a French translation during the past year may serve as a point of unification for a number of studies (22). As will be remembered, an attempt was made in that book to discover certain major taxonomic variables in personality by means of factorial techniques applied to objective test scores. The main dimensions discovered outside the cognitive field were those of neuroticism and of extraversion-introversion. These psychological dimensions were found to be related to body build in men, and the two recent studies by Rees referred to above show that similar correlations and factors can be found for women also. As previously, an index was derived from the factorial study of body measurement to determine a person's "body type," so that now we have available such an index for women as well as for men. The fact that such reproducible correlations exist between body type and the measured dimensions of personality suggests strongly that these dimensions are constitutional in origin; a more direct proof for this proposition will be given later. On the more definitely psychological side, Himmelweit & Petrie (43) have applied the type of objective personality test made familiar in *Dimensions of Personality* (19) to groups of normal and neurotic children in an attempt to discover whether the same factor of neuroticism could be isolated there also. The experiment was entirely successful and the "discriminant function" analysis of the two groups showed a relatively low level of misclassification, particularly when the lack of reliability of the psychiatric criterion is taken into account.

The factor of neuroticism also appears in two studies which are of interest here for rather different reasons. The first of these is a statistical study of the Rorschach test in which 100 Indian students were given the Rorschach test and verbal and nonverbal group tests of intelligence, as well as Cattell's test of fluency (88). In addition, each subject was rated independently by two judges on a number of personality traits. (The average reliability of these ratings was .71.) Tetrachoric correlations were calculated between 36 Rorschach scoring categories, and three main factors were extracted. The first of these was one of associative fluency, which correlated with the fluency test and with the rating for imagination. The second factor was one of intelligence, correlating quite highly with the two intelligence tests and also with a rating for intelligence. It is the third factor which is interesting in this connection; a correlation of .68 was found between Sen's third factor and the rating for neurotic tendency, which is the highest of all correlations recorded between factors and external criteria. We thus find

strong evidence in this factorial study of the Rorschach, both of the existence of the factor of neuroticism and the value of the Rorschach test in predicting the strength of this factor in a given individual.

The other factorial study to which attention is drawn here is one carried out by Reyburn & Raath (81). This experiment involved a rating by 83 observers of two subjects each, with relatively high reliability of the ratings. These ratings were on a five-point scale covering 45 well-defined personality traits. The table of intercorrelations was factor analysed and an oblique solution is reported which gave rise to six factors which are not independent of each other. The correlations between these factors clearly give rise to higher order factors which were not derived by Reyburn & Raath, but were calculated for the purpose of this review because of their theoretical interest. Thirteen iterations were required before the communalities began to converge. The results of the analysis are very clear-cut: there are two factors, the first one clearly the neuroticism factor (with a saturation of .97 on "stability"), and the second factor clearly an extrovert-introvert factor, with high positive saturations for "assertiveness" and "spontaneity" and negative ones for "sensitivity" and "inferiority."

We must now turn to quite a different type of investigation which still, however, falls under the general heading of personality organisation, namely, a book by the Gluecks (31) on juvenile delinquency. This 10-year study involving almost three dozen investigators is a monument to patient and painstaking accumulation of details relevant to the characterization of a particular type of person, in this case the juvenile delinquent, and to the equally painstaking analysis of the data. Not all of the book is relevant to psychology, and if those parts that are do bear witness to the aridity of such a collection of material when there is no guiding hypothesis underlying it, this may be due in part to the sociological and legal training of the investigators. Roughly speaking, 500 delinquent boys were matched with 500 nondelinquent boys on age, general intelligence, national origin, and residence in under-privileged neighbourhoods. Investigations of particular interest to psychologists were carried out by means of the Rorschach test and through the use of Sheldon's somatotyping technique, which again links this study with the constitutional work of Kretschmer, Martiny, and others. In addition to these data, psychiatric interviews were held with all the children. It is somewhat of a surprise to note that, although the two groups were matched with respect to intelligence, there is a significant difference (CR=3.57) on the Wechsler Verbal Scale between the two groups, a difference larger than most of those found with respect to "qualitative and dynamic aspects of intelligence" by means of the Rorschach. When we come to the more affective and conative aspects of personality, we learn from the Rorschach that the delinquents are significantly less suggestible and less neurotic than are the nondelinquents. From the psychiatrist we learn that the delinquents are much more suggestible and have far more emotional conflicts, as well as being less adequate with respect to their deep-rooted personal dynamics. The authors seem to feel that such contradictions may

make it difficult for the reader to find out precisely which of the two groups is more suggestible or which is more neurotic. They therefore point out that the psychiatrist and the Rorschach expert do not agree with respect to their definition of these terms. They say,

> The psychiatrist defines emotional instability as "a conflict of feelings," while the Rorschach trait of emotional lability is defined as "lack of affective inhibition in a quantitative sense."

It is difficult to understand either of these definitions, and it still remains puzzling why emotional instability and emotional lability should show a high negative correlation. Here, indeed, is a clear example of the absolute necessity of operational definition if any worth-while results are to be had from investigations of this kind.

One further point of interest relates to the Rorschach test. It is claimed that, in a blind analysis, a considerable number of cases were correctly diagnosed as delinquent or nondelinquent. Here, as elsewhere in the book, there is no recognition of the possibility, indeed the probability, that there are features in the protocol such as, for instance, the use of swear words, etc., which would make such recognition possible, quite apart from any formal Rorschach interpretation. Until such possibilities are ruled out, little attention can be paid to the results as reported.

Another set of data which is of interest is that related to bodily constitution. It is found that in gross bodily size, delinquents are superior to nondelinquents and that in delinquents there is a greater laterality of bodybuild. By and large, according to Sheldon's system, the delinquents tend to be mesomorphic, while the controls do not have any excess in that somatotype. This finding, indeed, we may regard as well established, and it is of particular interest in view of the force with which it points to the importance of constitutional factors in delinquency.

Development

Hypotheses regarding the development of personality have usually been either in terms of environment, in terms of heredity, or, very much more rarely, in terms of both plus an interaction factor. A quick glance through recent publications in this field reveals an overwhelming bias in most psychological and psychiatric writers in favour of environmentalistic hypotheses and a complete disregard of the possibility that the figures given in support of this may find an equally easy interpretation in terms of a nativistic hypothesis. Slater (92) has drawn attention to this bias in a very forcible manner:

> There has . . . been an increasing tendency among clinicians to minimize the effects attributable to genetical causes, and to teach a psychiatry in which they receive little or no mention. This tendency has been marked in Britain, but it has assumed formidable strength in the U.S.A. Instead of a harmonious development, in which the psychoses and neuroses, constitution and environment, psychogenesis and physiogenesis receive their due share of attention, interest among practical workers has been de-

XUM

voted more and more exclusively towards psychotherapy, psychoanalysis, social psychiatry, personnel selection, group therapy, and preoccupations with anthropology, sociology and political theory. In its one-sidedness, this development is not healthy.

It is a sign of bad omen that it is possible for text-books of clinical psychiatry to appear, with claims for comprehensiveness, in which no mention is made of the established facts of genetics and of the hereditary element in mental disorder. Their authors appear to feel, though in fact this view depends on a misapprehension, that recognition of a hereditary factor implies a therapeutic nihilism; and that an energetic and optimistic attitude towards treatment calls for a neglect of hereditary factors, just as a due appreciation of the patient as an individual demands forgetfulness of nosological entities. One suspects that the prime motivation is derived from the philosophy of Dewey, which is no doubt over-simplified in the notion that one should accept as true that which has convenient practical applications.

It would not perhaps be putting it too high to say that we are witnessing the manifestation of an anti-scientific tendency which is winning an increasing number of supporters. The customary canons of scientific reasoning are ignored by these schools. Uncomfortable facts are left unconsidered. Hypotheses are multiplied regardless of the principle of economy. Explanations which may be valid for certain members of a class of phenomena are regarded as true for the class as a whole. Interpretations which conform with theory, and which might be true, are regarded as established. Possible alternatives are not considered, and no attempt is made to seek for evidence of critical value which shall decide between them. Criticisms from outside are ignored, and only the initiate may be heard. Utterance is dogmatic and arrogant and lacks scientific humility and caution. These are the mental mechanisms which we associate with the growth of a religious orthodoxy, and not with the progress of science. The movement is of significance to genetics, because it is likely adversely to affect the personnel and facilities for research, and to lead to a psychiatry without biological foundation and divorced from contact with the other natural sciences.

The comparison of two groups, such as delinquents and nondelinquents as in Glueck's study, has often been used to extract information or test hypotheses regarding causes for differential development. An example of such a study may be found in a comparison by Warren (102) on 90 children suffering from conduct disorders and 70 children suffering from neurotic disorders. Some of the historical antecedents appear to have a close relation with eventual behaviour of these children. Some of the most interesting ones are related to maternal behaviour. Thus, oversolicitude on the part of the mother is present in 14 per cent of the conduct disorder group, but in 40 per cent of the neurotic disorder group. Undersolicitude, on the other hand, occurs in 40 per cent of the conduct disorder group and in only 7 per cent of the neurotic disorder group. Paternal over- and undersolicitude, whilst tending in the same direction, appears much less highly related to filial conduct. Overanxiety in the mother before the present disorder developed in the child was found in 39 per cent of the cases in the conduct disorder group, but in 71 per cent of the cases in the neurotic disorder group. Separation from the mother before the age of five was found in 46 per cent of the cases in the conduct group and in 16 per cent of the cases in the neurotic disorder group. Similar figures for the father are 69 per cent and 39 per cent,

respectively. All these figures are very highly significant and are discussed by Warren, under the heading of "Etiological Factors."

He also mentions certain constitutional factors which are interesting. Thus, for instance, epilepsy and psychopathy occur in 22 per cent of the family histories of the "conduct disorder" group and in only 11 per cent of the cases in the "neurotic disorder" group. Neuroses, on the other hand, are more frequent in the family histories of neurotic children (43 per cent) than in the family histories of conduct disorder children (32 per cent). These findings suggest an alternative conclusion to that of Warren. Instead of accounting for the observed correlation between solicitude or anxiety and disorder in terms of a direct causal relationship, we may prefer an interpretation in terms purely of constitutional factors, i.e., neurotic mothers have neurotic children and also show overanxiety, oversolicitude, etc. Epileptic and psychopathic mothers have children with conduct disorders; they also tend to show undersolicitude and little anxiety. The data given do not enable us to decide between these two hypotheses—indeed, it is possible that we may here be dealing with an interaction between heredity and environment. It is interesting, however, to note the ease with which the environmental hypothesis is accepted by most writers and readers in spite of the complete lack of direct evidence in its favour.

The fact that a mother's behaviour pattern (say, oversolicitude) is correlated with the child's behaviour pattern (say, neuroticism) may indeed by an expression of a direct causal relation. It is equally possible that both the mother's and the child's behaviour are genetically determined and that their correlation is due to the fact that both are correlated with this third variable. In nearly all the papers examined here, it will be found that there is no possibility of deciding between these two alternatives, yet nearly all the writers have explicitly accepted the environmentalistic hypothesis and implicitly rejected the hereditary hypothesis. In drawing attention in each case to the alternative possibility, I do not wish to be misunderstood as maintaining that this alternative hypothesis is more likely to be correct than the one presented by the writers concerned; in the absence of further evidence it is simply impossible to decide one way or the other. In such a situation, it is clearly unscientific to accept one hypothesis without even considering the possibility of an alternative hypothesis. Evidence strongly favouring the hereditary hypothesis with respect to at least one major dimension of personality will be discussed when we come to the last paper in this series.

Another research of much interest in connection with the development of personality is reported in two papers by Goldman-Eisler (32, 33), which take their cue from the Freudian hypothesis of oral types. Her procedure is of considerable interest from the methodological point of view. Taking the description of the orally gratified and the orally ungratified type from psychoanalytic literature, she has constructed a set of 19 questionnaire scales, each containing on the average eight items to measure these 19 traits. It was possible, from Freud's theory, to predict the way in which the scales should hang together and the kind of factor structure which should

XUM

emerge from their intercorrelations. Having shown that the factor emerging from the matrix of intercorrelations is indeed similar to the one posited by Freudian theory, the author goes on to the second part of her task which is to test the hypothesis that time of weaning is causally related to oral gratification or lack of oral gratification. Using factor scores for almost 100 adult subjects on the oral gratification factor and information obtained from the mothers of these subjects as to the age at which they were taken off the breast, an analysis of variance was carried out on the scores of the early and late weaners, defining early weaning as weaning before the age of four months, and late weaning as weaning at five months of age, or more. A correlation of .3, fully significant at the 1 per cent level, was found between oral optimism and late weaning.

This study, exemplary as it is of its kind, also suffers from its failure to consider the hereditary hypothesis. It is quite conceivable that an innate factor may cause both early weaning and the personality trait denoted as "oral pessimism"; the correlation between these two may be entirely fortuitous. It may be objected to such an interpretation that seemingly the author has gone through all the steps recommended by writers on scientific methodology, i.e., she has taken a hypothesis, made a deduction from this hypothesis, and verified the deduction. Such an objection, however, would hardly be valid because, as so often in psychology, it would take the empty shell of scientific methodology for the real thing. The hypothesis itself, in the case of Freud, was, on his own showing, the result of the observation that early weaning and "oral pessimism" coincide; if the hypothesis is based on such an observation, then clearly the verification of the observation cannot be used to verify the hypothesis. It is only when deductions are made from hypotheses, such that they lead to new observations, that we can really talk about verification in the scientific sense. This should not be taken to detract from the value of Goldman-Eisler's study, which has added two important facts to our knowledge of personality structure and development; it should rather act as a spur to devise experiments which take into account alternative hypotheses which must be considered before we can accept the environmentalist's hypothesis.

Rather similar to Warren's study, but more rigorous in its criteria of selection, is an article by McKeouwn (62) on the behaviour of parents of schizophrenic, behaviour problem, and normal children. Among the schizophrenics, the parents of the same sex showed a marked incidence of dominant, antagonistic behaviour; the same type of behaviour is shown by both parents among the behaviour problem children. Encouraging behaviour is rare among parents of schizophrenic and behaviour problem children, but is predominant among the parents of normals. It is hardly necessary to repeat our general criticism of conclusions drawn from such statements; the results are quite as explicable in terms of heredity as of environmental influences.

More cautious in its interpretations is a review of intrafamily resemblances in personality characteristics (82). (*a*) The evidence goes to show

that the resemblances of both sons and daughters to mothers may be higher than that of fathers when the children are immature; this difference, however, tends to disappear at maturity. (*b*) When a test is decidedly more appropriate to one sex than the other, greater similarity is usually found for the more adequately measured pairs. (*c*) On measures of attitude and information, daughters tend to resemble both parents more than do sons. (*d*) There is no indication on any variable that mother-son or father-daughter resemblance is greater than that for other parents. These results, while they throw a limited amount of light on the Freudian hypothesis, are, of course, neutral with respect to the environmentalist-nativistic discussion.

Another study with a somewhat ambitious title "The Influence of Constitution and Environment on the Development of Adopted Children" (72) is much too sketchy to make any appraisal possible, but as far as can be seen, it has avoided none of the pitfalls to which such studies are exposed. Another writer (57), who bases his conclusions regarding the greater influence of environmental factors on personality on a study of three pairs of identical twins, uses a somewhat anecdotal method, which can hardly be regarded as likely to impress the scientific investigator.

We must now turn to a strictly experimental study in which an attempt was made to give numerical expression to the extent to which hereditary influences can be said to account for the total variance of a particular operationally defined personality trait (26). This study, which begins with a review of the work done with monozygotic and dizygotic twins in the field of personality, makes a fundamental criticism which applies not only to the studies reviewed but also to twin studies in the cognitive field. The argument runs roughly like this: The typical experiment in this field attempts to find an answer to a rather general question, such as, for instance, the relative contribution of heredity and environment to individual differences in intelligence. The experiment is set up by giving a particular test, such as the Binet, to groups of monozygotic and dizygotic twins and using a particular statistical technique, such as, for instance, Holzinger's h^2 (71) to give a numerical answer to the original question. It is pointed out that this procedure is quite fallacious because it equates "intelligence" with "Binet score." This objection is made more explicit by reference to the factorial equation of a given test. Studies by McNemar (64) and Burt & John (10) have shown that only about 40 per cent of the total variance of the Binet test is accountable for in terms of a general factor of intelligence, leaving the remainder, i.e., over half of the total variance, to be accounted for in terms of verbal, numerical, memory, and other group factors, as well as, of course, a specific factor. Now a finding that the Binet test score is inherited to the extent of 80 per cent throws no direct light on the inheritance of intelligence. It may be that 40 per cent of the total Binet variance, which is a measure of "g", is inherited 100 per cent and that the group and specific factors are inherited to only a very limited degree. Alternately, it is possible that "g" is inherited to a very limited extent indeed, while the group and specific factors are inherited 100 per cent. In other words, no conclusion can be

drawn from a specific test to the inheritance of a trait measured imperfectly by that test. It follows from this argument that a completely different methodology is called for, and this methodology will become apparent in the description of the experiment itself.

A battery of tests which had been previously shown to be good measures of the personality trait of neuroticism were given to 25 pairs of monozygotic and 25 pairs of dizygotic twins, aged between 11 and 13. Scores on these tests for the total group of 100 children were intercorrelated, and a factor analysis carried out using the method of criterion analysis recently proposed for experimental studies in the field of personality (23). (As a control group, 21 neurotic children were used in order to find a unique and invariant solution to the problem of rotation.) Intraclass correlations were computed for monozygotic and dizygotic twins separately on each of the 17 tests used as well as on the factor scores of the children derived for the general factor of neuroticism. Also calculated for each test and for the factor score was Holzinger's h^2 statistic, purporting to give an estimate of the percentage of the total variance contributed by heredity. The intraclass correlation of the neuroticism score was .85 for monozygotic twins and .22 for dizygotic twins, giving an h^2 of .81, thus indicating, if we can indeed make the assumptions on which Holzinger's h^2 is based, that 81 per cent of the total variance of this trait is due to hereditary influences. This result is not subject to the objection mentioned previously as now we are not dealing with an individual test but with a factor score.

From these data another important conclusion can be drawn. It is found that the h^2 for any one of the 17 tests used is considerably smaller than the h^2 for the neuroticism factor. This shows conclusively that this factor is not a statistical artifact but has very definite biological reality, a demonstration of particular value in view of the frequent criticisms of factor analysis made on this score. Appended to the article is a note dealing with the assumptions underlying Holzinger's h^2 statistic (66) which makes it appear very doubtful, to say the least, that the value of 81 per cent given above can be regarded as more than a very rough approximation to the true value. However, it does appear clear from this study that heredity plays a very strong part in the determination of personality differences and that psychology cannot go on making the easy and implicit assumption, which is inherent in both psychoanalysis and behaviourism, that environment is the only major variable determining individual differences. This belief, which has acquired a mystical and almost religious fervour in the U.S.S.R. is also widespread in democratic countries, for reasons which, as Pastore (73) has pointed out, may not be entirely apolitical. It seems a great pity that scientific issue should be clouded in this fashion.

Methodology and Measurement

In a sense, it may be said that personality research stands and falls with the adequacy, or otherwise, of its methodology. The many contradictory findings which are published in the literature are almost entirely due to differ-

ences in methodology, and it would be a great boon to science if editors of psychological journals would firmly refuse to print papers containing obvious methodological shortcomings. An example of the differences in results which may be found when methodological safeguards are neglected is apparent in two papers reporting work on the influence of glutamic acid on human intelligence. One of these (35) arrived at the conclusion that "glutamic acid has beneficial effects upon mental age, personality, and school achievement" on the basis of a research design so chaotic and lacking in controls as to make any conclusion impossible. The other (69), using a proper experimental design with control groups, came to the conclusion that "the results of the cognitive tests provided no evidence in favour of the hypothesis that glutamic acid improves cognitive functioning." When, even in a case where proper tests are available and research design can follow the models in any elementary textbook, differences such as these can still arise, it is not to be wondered at that in the more complex fields, where no outside criteria are available and tests of much lower reliability and no known validity are used, there should be contradictions and differences of opinion (20). Ultimately, a solution to problems in the field of personality depends more on advances in methodology than on almost any other developments.

The most impressive experiment under this heading is the Michigan study of the prediction of success in the Veterans' Administration training programme in clinical psychology (47, 48, 49). The large number of subjects studied, the very large number of techniques of measurement and assessment used, and the excellence of the experimental design would have made the findings from this programme outstanding, even if they had been less revolutionary in their import. From every point of view, the final results are a devastating comment on and criticism of the clinical methods of interviewing and projective testing current in most applied personality work. It was found that the most efficient clinical predictions in terms of both validity and economy of data are those based only on the matters contained in the credentials file and in the objective test profiles. The addition of autobiographical and projective test data appears to have contributed little or nothing to the validities of the assessment ratings. Neither the initial nor the intensive interviews made any apparent contribution. In fact, the predictions based on the credentials and objective tests are better than those made at the end of the programme on the basis of test procedures and observations! This consistent trend would seem to be all the more significant in view of the fact that assessment staff members tended to be uniformly of the opinion that the interview contributed most to their "understanding of the case," followed by either the projective test or an autobiography.

There are many other interesting and important findings [Kelly & Fiske (49)]:

> The pooling of clinical judgments and the staff conference does not seem to increase the validity of predictions.
>
> Predictions based on individual projective tests as well as those based on an integration of data from all four projective tests yielded relatively low correlations with

the rated criteria. Scores from a single objective test obtainable by mail at little cost predicted each of several criteria as well as all the clinical judgments made in the entire assessment program.

Some of the comments made by the authors of the papers quoted draw attention to the main findings. They point out that

many who have seen our results have been disturbed by the findings regarding the validity for this selection problem of specific techniques, which are felt by many professional psychologists to have a high degree of face validity (or is it faith validity)? Thus, it was the firm conviction of the staff of the OSS Assessment Program (98) that the global evaluation of a person permits much more accurate predictions of his future performance than can possibly be achieved by a more segmental approach. . . . Our own findings to date serve to raise doubts concerning the validity of this general proposition. . . . Although the unstructured interview is one of the most widely used tools in personnel selection, the writers know of no evidence in the literature to suggest that such interviews have other than extremely low validity, which hardly justifies the degree of confidence and esteem with which they are held by users of the interview.

One aspect of our findings is most disconcerting to us: the inverse relationship between the confidence of staff members at the time of making a prediction and the measured validity of that prediction.

Kelly & Fiske (49) advance what seems the most reasonable hypothesis to account for these findings.

The essence of clinical evaluation and integration of data involves permitting the clinician to assign to each item of opinion "beta weights," which vary from case to case according to the clinician's perceived patterning of the data. Our findings suggest that this technique may result in increasing the ratio of error variance to true variance with successive ratings based on increments of information. This may lead to a subjective feeling of increased knowledge about the assessee without a parallel awareness of the fact that many of the additional items of information are not actually correlated with the criteria, and hence should not be weighted in arriving at a prediction about the assessee.

These words, which would at the same time explain the singular finding of the OSS staff that a one-day assessment programme gives better predictions than a three-day programme (98), should be engraved over the portals of every psychiatric institution and psychological department that attempts to apply scientific methods to problems of personality!

Signs are not missing that the factual results of Kelly & Fiske and the frequent strictures of the more experimentally minded are succeeding in making users of projective tests and other clinical methods more conscious of weaknesses in their methodology. The number of those who believe with Harrison (36), Lersch (58) and Heiss (38, 39, 40) "that clinical validation is ample evidence for the present with respect to the validity of projective tests" is getting smaller, and the number of those who, like Schneider (85), insist that

Rorschach validating procedures can be most fruitfully treated as problems in relating Rorschach variables to independent measures of component personality processes

is increasing, as shown not only in Schneider's summary of Rorschach validation methods, but also in Mensh's paper on statistical techniques in present day psychodiagnostics (68). Other examples are the application of factor analysis to Rorschach diagnostic signs by Hughes (46) and Wittenborn (103), the attempts by Blum (4) and Goldman-Eisler (32, 33), mentioned in the previous section, to submit psychoanalytic concepts to experimental verification, and the attempts by Rosenzweig (84) and Luft (59) to make the process of hypothesis formation and verification in clinical work more explicit.

Of particular importance in this field of methodology and measurement is the use of factorial techniques and their development along novel lines. Among those following the traditional methods of Thurstone, none perhaps has so continuously and assiduously applied himself to the description of the personality sphere in factorial terms than has Cattell. The recent publication of two of his textbooks (11, 13) makes it possible to follow in detail, not only his own contribution to this field, but also the way in which he believes it to fit into a general theoretical framework. Among modifications of traditional techniques, we may perhaps mention Stephenson's "Q" technique (97) and Eysenck's method of criterion analysis (23).

Cattell's presentation is organized in terms of his factorial studies of ratings, and he brings forward much evidence from independent studies, again largely of a verbal kind, to support his contentions. From this point of view, his book is an excellent summary of the important contributions he has made to personality measurement and duplicates much of what has already appeared in his book on the *Description and Measurement of Personality* (12). He does not, however, effectively answer certain objections to his approach which have been made in the past. Thus, it has appeared to many that all descriptions of personality which are based largely on ratings, made by relatively untrained subjects, must inevitably partake of the unreliability and lack of certainty which attaches to such ratings. If it be true that a rating is almost as diagnostic of the rater as of the ratee, then what we are dealing with in our analysis is a conglomeration of rater-, ratee-, interaction-, error-, halo-, and other variances, so that it is very difficult to see how any rigid scientific structure can be built on such shifting foundations. Cattell, of course, is aware of these difficulties and has tried in a recent paper (14) to link up his rating studies with objective behaviour tests; little success, however, appears to have attached to this endeavour. It is too early yet to come to any final decision on the value of Cattell's results; what is particularly welcome in his approach, however, is the constant striving after taxonomic principles and his endeavour in doing so to use rigorous statistical methods. If the material on which he uses these methods is not always of a very high order, perhaps blame for that attaches to psychology as a whole rather than to him as an individual.

It is when he says that

> observations on the development of dynamic structures in personality necessarily remain to-day still largely at a clinical level, but they are in major outlines sufficiently well established to permit us to go forward to a systematisation

that the present writer must part company with him. It is disappointing to see standards of rigour and of scientific exactitude, which are stressed in the first part, thrown away so easily and nonchalantly in the second part, and if, indeed, we are to agree with Cattell that observations at a clinical level are sufficient for the type of systematisation on which he embarks, then we may perhaps justifiably ask why we should undergo the long and difficult discipline of step-by-step scientific analysis in the demand for which he is so imperative earlier on. Nor does Cattell's actual systematisation convince that this particular solution is on any but a verbal level. While not denying the difficulty of integrating this type of material with scientific methodology, I cannot feel that Cattell's approach helps to solve this difficulty. Indeed, the systematisation of insufficient data may very easily lead the unwary to an unjustified belief in the excellence of the whole structure.

Stephenson's "Q" technique, which was introduced by him about 15 years ago under the title of "Inverted Factor Technique" (94, 95, 96) is difficult to assess for various reasons. In the first place, the practice of correlating persons instead of tests is one which was familiar to psychologists for many years already when Stephenson's articles appeared; the novelty of the procedure, therefore, can hardly reside in this particular aspect of it. Yet, when we look for other aspects of this technique, it is difficult to find them stated unambiguously and unequivocally; to be told that the "Q technique . . . is a system of devices which . . . serves to affirm conclusions already reached but requiring proof" seems neither particularly enlightening, nor, when we look at the examples provided by Stephenson, particularly true. Another difficulty lies in Stephenson's failure to deal consistently and stringently with a number of questions which require to be answered with regard to his system, such as, for instance, questions relating to sampling (both of persons and of traits) and questions relating to rotation. Presumably, the factors arrived at by means of this technique are as meaningless and lacking in invariance without rotation as are factors extracted by any other technique. Another difficulty faced by Stephenson, which he has not dealt with convincingly, is the question of a metric. Correlations can only be run over a metric continuum; can the type of data to which Stephenson applies his method be regarded as giving such a metric continuum when the summation is over people rather than over tests? It is impossible to anticipate Stephenson's replies to these questions from the few small, illustrative *ad hoc* experiments he has published; these may whet one's appetite but do very little to assuage one's doubts regarding claims such as that this technique "neatly represents the self even if in a statistical fashion," or that "it provides . . . by far the best framework into which to fit typology in general." We must be reassured on these important points, and we must also have some evidence that the factors arrived at by means of "Q" technique are unique and invariant. Finally, I think we would require evidence, which has hitherto not been forthcoming, that the factors discovered in this way differ essentially from those which would appear in an orthodox type of factor analysis, because Burt has shown rather convincingly (9) that there is

no essential difference between the two solutions and that factors from one can easily be translated into factors from the other.

The third application of factorial design to personality study is Eysenck's use of criterion analysis in an attempt to isolate the fundamental "dimensions of personality." A recent paper (21) has set forth the results of using this method in order to solve a number of theoretical and practical problems. Applications include the prediction of employability of mental defectives (successful at a remarkably high level) (100, 101), the prediction of working efficiency of nursing trainees (78), and the prediction of examination success of medical and other students (41, 44, 77). In all of these areas, the addition of objective personality tests of the kind used to define operationally the concepts of "neuroticism" and "introversion" to the more usual measures of ability improved prediction to a considerable extent.

Of more theoretical importance is the work of Petrie (74, 75, 76) on the aftereffects of lobotomy. Using as her starting-point the hypothesis that interference in neurotic patients with the frontal lobes would lead to changes along both these dimensions (towards a decrease in neuroticism and an increase in extraversion), this investigator used objective personality tests previously validated as measures of these factors. Changes of an extra-chance character were observed along the lines of the hypothesis, which was therefore considered substantiated. These important studies show clearly the superiority of investigations based on cogent hypotheses over the more widely used "buck-shot approach," which consists of the unsystematic use of many varied but unrelated tests, selected without the benefit of any guiding hypothesis.

Theory

Attention has already been drawn to certain theoretical battlefields where issues are being fought over which, in spite of their great antiquity, will presumably engage psychologists for a long time to come (1). Some of these, like the environmentalist-hereditarian dispute, are perhaps incapable of being solved in terms of those concepts which still determine our thinking. Others, such as the organismic versus analytic approach may have become appreciably nearer solution through work such as that of Kelly and his assistants at Michigan (49). In this section, however, I shall deal not so much with these very large and complex fields, but rather with the less extensive type of theory which can be handled experimentally so much better and which, in the long run, will presumably be rather more fruitful.

One of the most impressive areas of advance, during the past year has been that linking perception with personality. In part, of course, this interest in perception is simply a fashion. Where 30 years ago interest centred on autonomic measures, particularly the psychogalvanic reflex, and where later on it switched to expressive movements and the Luria type of experiment, we now have perception in its various aspects. [That these older methods have not lost all interest to psychologists is indicated by papers by Duffy (17), drawing attention to the value of the psychogalvanic reflex as

a measure of energy mobilization, and McCurdy (61), drawing attention to the correlation between "consciousness and the galvonometer."]

Concentration on one of these three major approaches to personality may have its advantages, but the student of personality should not let himself be carried away by enthusiasm for any one method of approach. This having been said, we must acknowledge that many of the ideas, hypotheses, and advances in recent years by writers whose main interest has been in perception have been ingenious and fruitful. They have led to new experiments and to the discovery of new facts, which in turn have led to new hypotheses. The whole movement certainly promises to make a very real and lasting contribution to the study of personality.

Among the publications which mark this interest, pride of place must go to the appearance in book form of the Clinical Psychology Symposium held in 1949 to 1950 at the University of Texas (3). Edited by Blake & Ramsey, this book, on *Perception: An Approach to Personality*, contains much valuable information and novel material as well as a good deal of material which is obscure, philosophical, and nonscientific. Of main interest in this context are papers by Bruner (8), Klein (50), and Frenkel-Brunswik (29), dealing respectively with "Personality Dynamics and the Process of Perceiving," "The Personal World Through Perception," and "Personality Theory and Perception."

Bruner analyses perception in terms of hypotheses: "An operational definition of *hypothesis* can be stated by reference to the specific selectivity of a given perception at a given time." The basic property of hypothesis is what he calls "strength." Three theorems are developed to deal with this concept:

(1) The stronger a hypothesis the greater its likelihood of arousal in a given situation.

(2) The greater the strength of a hypothesis, the less the amount of appropriate information necessary to confirm it.

(3) The greater the strength of a hypothesis, the more the amount of inappropriate or contradictory information necessary to infirm it.

I cannot follow through the brilliant development of this theme by Bruner, which serves to integrate a large amount of experimental material; I can only say that it seems to contain the seeds for really important advances in this field.

Klein will probably be found in general agreement with Bruner's general approach, although where Bruner's stress is on general laws, Klein's is more on individual differences. This is expressed in his key concept of *Anschauung* or "attitude,"

A perceptual attitude is a personal outlook on the world, embodying in perception one of the ego's adaptive requirements. A style of reality-testing is expressed through it. It expresses a broader control principle which makes comparable demands upon other systems besides perception.

Of particular interest are the experiments which follow from this theoretical approach and which are reported in terms of what purports to be a dimen-

sional analysis, although one sadly misses the statistics appropriate to this type of analysis. However, the experiments themselves and the hypotheses expressed in them are brilliantly conceived and carried out, although there is no space here to describe them in any detail.

Frenkel-Brunswik's paper has a more psychoanalytic bias, but again the emphasis is on experimental testing and on perceptual manifestations, and this author, too, reports new methods of approach which should be of great interest to the experimentalist. From her point of view, she would perhaps regard the whole book as an indication of the tendency to shift interest from the id and the super ego towards the ego, a point also emphasised by Bronfenbrenner in a more general, but very valuable chapter, "Toward An Integrated Theory of Personality" (7).

Another area in which current interest is considerable is that of the application of learning theory to the concept of personality. Here again, instead of discussing in detail the somewhat bewildering mass of papers, we will discuss in greater detail one important publication, namely, Mowrer's book on *Learning Theory and Personality Dynamics* (70). This is, in essence, a collection of papers published over the years, but, as Gestalt psychologists might have pointed out, the whole is often less than the sum of the parts, and what may appear brilliant and suggestive in a single paper, may appear much more questionable and subject to doubt when forming part of a larger structure. Mowrer's main points are the following. In the first place, he insists that there are two basic learning processes: one of these which he calls "problem solving" occurs when a primary or secondary drive is reduced, the other which he calls "conditioning" occurs on the basis of contiguity, or double stimulation, and accounts for the acquisition of the secondary drive. In the second place, fear ("anxiety") is not conceived of as having primarily an inhibitory function but as a drive, and as such provides reinforcement through drive reduction. These conceptions are exemplified in a number of experimental papers and are applied to a large number of putative psychoanalytic processes.

In spite of the great interest which must obviously be attached to such a long sustained attempt to integrate learning theory and personality dynamics, there are certain obvious criticisms which must be made of this work. In the first place, it is interesting to see that although the first eight papers contain a monistic theory of learning, whereas the later ones adopt a dualistic approach, yet the author apparently finds it quite easy to reconcile his data in retrospect with the new theory. If that is possible, it appears somewhat difficult to take either of these two approaches very seriously. If it does not matter very much which approach is used, then presumably there is a lack of rigour somewhere which is somewhat disturbing. This lack of rigour comes out again in Mowrer's treatment of latent learning where secondary drives, in their hypothetical reduction in latent learning, are said to provide rewards which are no less potent than those provided, for example, by hunger reduction. In the absence of any independent measurement of such secondary drives, this is a typical *ad hoc* hypothesis which can do

very little to shore up this particular theoretical edifice. [Contrast with Mowrer's position, for instance, the conclusion of Thistlethwaite's recent review of latent learning (99).]

In the second place, Mowrer's explanation of the neurotic paradox appears to leave out considerations which are quite vital. (He defines the "neurotic paradox" in terms of the fact that in neurotics, actions which have predominantly unfavourable consequences persist over a period of months or years, although, according to ordinary learning theory, such nonadaptive behaviour should be dropped.) Mowrer, in setting up experiments to illustrate this paradox, falls into an error common to many animal psychologists and pointed out many years ago by Köhler (51); he structures the test situation in terms of his own perceptual and conceptual field rather than in terms of the rat's. As an example, we may quote an experiment in which the rat is taught to run from an electrified cage to a nonelectrified shelter; Mowrer shows that when the part of the cage where the rat is put down is not electrified, after a period of training, the rat will, nevertheless, run across an electrified part of the cage in order to get to the shelter. This is given as an example of paradoxical behaviour because a rat could have adjusted better by just staying in the nonelectrified part of the cage. While this is, of course, true in terms of the experimenter's perception, who knows the exact set-up of the experiment, the behaviour of the rat in terms of his knowledge and perception seems quite reasonable and nonparadoxical; indeed, a human subject put in the same position might reasonably argue, "I always get shocked in this part of the cage but I'm safe over there, so let's get the hell out of here, even if there is a momentary shock in getting to the shelter." This would be a cognitive explanation of the rat's behaviour in contradistinction to Mowrer's psychoanalytic one, and the fact that when he perceptually disrupted the perceptual field of the rat by introducing a dividing line between the electrified and the nonelectrified part of the cage, he found that this profoundly influenced the rat's behaviour, strongly supports this rival hypothesis. I have no space to discuss this criticism further, but when seen in the light of the perceptual theories discussed above, it will be seen that Mowrer has left out of account a most important hypothesis which could account for his data in many cases better than the hypothesis he offers.

I have been critical with Mowrer's book, not because it can be regarded as anything but an important contribution, but because he attempts to objectify and investigate experimental concepts which are of the utmost importance in personality theory. The highest standards of criticism should be applied to this type of work precisely because of its fundamental importance. It may be possible to link learning theory with personality and such a feat would certainly unify psychology more than almost any other advance, but Mowrer's attempt to do so suffers from weaknesses which must be expunged before it can be regarded as likely to lead in the right direction. What is true of his book is *a fortiori* true of other writers who fall short of his own ingenuity and high standard of experimental accuracy. How much

more could this book have advanced psychological insight, if, instead of republishing old papers, Mowrer could have sat down to think out the whole problem afresh and have written a new and critical exposition!

Among other interesting developments in the theory of personality is the publication by Gilbert (30) of a summary of German theories of stratification of personality. There is probably little value in attempting to abstract here the very concise version that Gilbert gives of this particular movement. Its importance is difficult to evaluate, but it does not seem to have given rise to any worth-while experimental studies, and its terms and concepts appear to be subject to the same objections that have so often been advanced against the concept of "instinct." Highly sophisticated theorisation at the semantic level seems to be designed to cover the failure to arrive at operational concepts or to submit theories to rigorous experimentation. In the 25 years that this type of theorising has been current in Germany, one would surely have expected it to produce or give rise to some modicum of experimentation, but instead, the same old concepts and words are tumbling around in the same old empty drum to the resounding echo of excessive verbalisation.

Much more in touch with current experimental work is Ross Stagner's attempt to use homeostasis as a unifying concept in personality theory (93). Taking his cue from Cannon's investigations on physiological constancy, he goes on to extend the same concept to perceptual constancy and even ego constancy. His approach can easily be integrated with that of the writers mentioned previously in connection with research on perception and personality. This type of theorising, which is in close touch with experimentation and serves to unify concepts from different fields, is of undoubted use in psychology, although the exact value of Stagner's concept can only be determined when attempts have been made to use it as a guide to further experimentation.

Neurological theorising is well represented during the past year by Krech's three papers dealing with dynamic systems as hypothetical constructs (53, 54, 55). Krech objects to current tendencies either to neglect neurology in creating hypothetical constructs, which, following MacCorquodale & Meehl (60), he contrasts with intervening variables, or to become subservient to and limited by the present unsatisfactory state of neurological knowledge. He quotes Bertalanffy (2) and Brillouin (5) to show that physics is not necessarily inimical to his attempt to deal with dynamic systems as open neurological systems, and he devotes considerable space to discussing the second law of thermodynamics. He then lays down three general specifications of dynamic systems which are concerned with the pattern of activities, the open system characteristics, and the locus of dynamic systems. These rules are too general to have any direct specific application, nor does it seem possible to derive deductions from them which could be tested directly so as to support or contradict them; however, they may serve to suggest methods of approach which are somewhat different from those currently explored by physiological psychologists, and, no doubt, future

papers will make this system more relevant to the direct experimental attack. All that can be said at the moment is that Krech's attempt, coinciding as it does with the work of Köhler & Wallach (52) and Hebb (37), may serve to rejuvenate this part of psychology which had begun to show signs of arteriosclerosis, if not senile dementia.

It may perhaps be said in summary of this section that theoretical work is valuable in so far as it pulls together experimental researches and points out similarities and correlations that might otherwise be overlooked. When it is a question of rather grandiose analogies, *ad hoc* hypotheses, and intuition, theorising can be harmful by distracting attention from soluble problems to philosophical speculation. Current publications contain examples of both and attention has been drawn to examples which illustrate either approach.

Summaries and Textbooks

There are many chapters relevant to the psychology of personality in *Recent Progress in Psychiatry* (28), particularly those on psychiatric genetics (92), physiological psychology (34), intelligence testing (6), and personality tests (24). Cattell's textbooks, already mentioned, summarize a good deal of the literature (11, 12, 13). Much of what is printed in *Current Trends in the Relation between Psychology and Medicine* is also relevant in this context (16). Summaries on particular problems in the field of personality appear in Blake & Ramsey's book on *Perception: An Approach to Personality* (3) already discussed. With respect to the problem of anxiety, there is a book of that title by Hoch & Zubin (45), as well as a more historically oriented one by May (67). A summary of the much neglected theories of Janet is given in a fairly clear exposition by Schwartz (87).

Certain aspects of personality, such as, for instance, those associated with aggressiveness, are dealt with in a recent symposium on *The Psychological Factors of Peace and War* (25, 42), while other social aspects of personality are touched on in a publication devoted to papers read at the University of Oklahoma Conference on *Social Psychology at the Crossroads* (83). In a more limited field, there is an excellent review by Crown on the aftereffects of prefrontal lobotomy (15), which lays particular stress on methodology in its discussion of published work.

Much interesting material on psychology and personality along lines radically different from ours is given by Wortis (104) and by McLeish (63) in their writings on Soviet psychology and psychiatry. The book by Wortis is particularly valuable, not so much for its comments, but for the translations of reports, directives, and criticisms emanating from Russian sources. These paint a somewhat horrifying picture of state interference in scientific matters, deliberate misrepresentation of Western points of view, complete ignorance of advances made during the past 30 years, and an intermingling of politics and science, which, while it is no doubt intentional and in line with Marxist thought, does not seem to be favourable to the production of original work in this field.

LITERATURE CITED

1. Allesch, J. von, *Psychol. Rundschau*, **1,** 4–16 (1949)
2. Bertalanffy, L. von, *Science*, **111,** 23–29 (1950)
3. Blake, R. R., and Ramsey, G. V., Eds., *Perception: An Approach to Personality* (Ronald Press Co., New York, 442 pp., 1951)
4. Blum, G. S., *J. Abnormal Social Psychol.*, **45,** 536–37 (1950)
5. Brillouin, L., *Am. Scientist*, **37,** 554–68 (1949)
6. Brody, M. B., and Williams, M., *Recent Progress in Psychiatry*, 163–96 (Fleming, G. W. T. H., Ed., J. & A. Churchill, Ltd., 711 pp., London, 1951)
7. Bronfenbrenner, U., *Perception: An Approach to Personality*, 206–57 (Blake, R. R., and Ramsey, G. V., Eds., Ronald Press Co., New York, 442 pp., 1951)
8. Bruner, J. S., *Perception: An Approach to Personality*, 121–47 (Blake, R. R., and Ramsey, G. V., Eds., Ronald Press Co., New York, 442 pp., 1951)
9. Burt, C. L., *The Factors of the Mind* (Univ. London Press, London, 509 pp., 1940)
10. Burt, C. L., and John, E., *Brit. J. Educ. Psychol.*, **12,** 139–87 (1942)
11. Cattell, R. B., *An Introduction to Personality Study* (Hutchinson's Univ. Lib., New York, 235 pp., 1950)
12. Cattell, R. B., *Description and Measurement of Personality* (G. G. Harrap & Co., Ltd., London, 602 pp., 1946)
13. Cattell, R. B., *Personality: A Systematic Theoretical and Factual Study* (McGraw-Hill Book Co., New York, 689 pp., 1950)
14. Cattell, R. B., and Saunders, D. R., *J. Social Psychol.*, **31,** 243–60 (1950)
15. Crown, S., *J. Mental Sci.*, **97,** 49–83 (1951)
16. Dennis, W., *Current Trends in the Relation Between Psychology and Medicine* (Univ. Pittsburgh Press, Pittsburgh, 189 pp., 1950)
17. Duffy, E., *Psychol. Rev.*, **58,** 30–40 (1951)
18. Eysenck, H. J., *J. Personality*, **19,** 123–52 (1950)
19. Eysenck, H. J., *Dimensions of Personality* (Kegan Paul, Trench, Trubner & Co., Ltd., London, 308 pp., 1947)
20. Eysenck, H. J., in *Proceedings of the 1929 Invitational Conference on Testing Problems*, 45–50 (Buros, O. K., Ch., Educ. Testing Service, Princeton, 1949)
21. Eysenck, H. J., *Travaux de la Semaine Internationale D'Anthropologie Differentielle* (Presses Universitaires de France, Paris, 1951)
22. Eysenck, H. J., *Les Dimensions de la Personnalité* (Presses Universitaires de France, Paris, 314 pp., 1950)
23. Eysenck, H. J., *Psychol. Rev.*, **57,** 38–65 (1950)
24. Eysenck, H. J., *Recent Progress in Psychiatry*, 197–253 (Fleming, G. W. T. H., Ed., J. & A. Churchill, Ltd., London, 711 pp., 1951)
25. Eysenck, H. J., *The Psychological Factors of Peace and War*, 49–81 (Pear, T. H., Ed., Hutchinson & Co., Ltd., London, 1950)
26. Eysenck, H. J., and Prell, D. B., *J. Mental Sci.*, **97,** 441–65 (1951)
27. Fisher, R. A., *Statistical Methods for Research Workers* (Oliver & Boyd, Ltd., 354 pp., 1948)
28. Fleming, G. W. T. H., Ed., *Recent Progress in Psychiatry* (J. & A. Churchill, Ltd., London, 711 pp., 1950)
29. Frenkel-Brunswik, E., *Perception: An Approach to Personality*, 356–420 (Blake, R. R., and Ramsey, G. V., Ed., Ronald Press Co., New York, 442 pp., 1951)
30. Gilbert, A. R., *J. Psychol.*, **31,** 3–20 (1951)

31. Glueck, S., and Glueck, E., *Unraveling Juvenile Delinquency* (Commonwealth Fund, New York, 399 pp., 1950)
32. Goldman-Eisler, F., *J. Personal.*, **17,** 83–103 (1948)
33. Goldman-Eisler, F., *J. Personal.*, **19,** 189–196 (1950)
34. Golla, F. L., *Recent Progress in Psychiatry*, 132–147 (Fleming, G. W. T. H., Ed., J. & A. Churchill, Ltd., London, 711 pp., 1951)
35. Harney, M., *Studies Psychol. Psychiat. Catholic Univ. Am.*, **8,** 1–61 (1950)
36. Harrison, R., *J. Psychol.*, **15,** 49–74 (1943)
37. Hebb, D. O., *Organization of Behaviour: A Neuropsychological Theory* (John Wiley & Sons, Inc., New York, 335 pp., 1949)
38. Heiss, R., *Psychol. Rundschau*, **1,** 266–75 (1950)
39. Heiss, R., *Psychol. Rundschau*, **2,** 9–19 (1951)
40. Heiss, R., *Psychol. Rundschau*, **2,** 63–75 (1951)
41. Himmelweit, H. T., *Brit. J. Sociol.*, **1,** 328–46 (1950)
42. Himmelweit, H. T., *The Psychological Factors of Peace and War*, 159–191 (Pear, T. H., Ed., Hutchinson & Co., Ltd., London, 1950)
43. Himmelweit, H. T., and Petrie, A., *Brit. J. Educ. Psychol.*, **21,** 9–29 (1951)
44. Himmelweit, H. T., and Summerfield, A., *Brit. J. Sociol.*, **2,** 59–75 (1951)
45. Hoch, P. H., and Zubin, J., *Anxiety* (Grune & Stratton, Inc., New York, 254 pp., 1950)
46. Hughes, R. M., *J. Gen. Psychol.*, **43,** 85–104 (1950)
47. Kelly, E. L., *Progress Rept., Univ. Mich.*, 1–14 (Dec., 1949)
48. Kelly, E. L., *J. Clin. Psychol.*, **3,** 39–42 (1947)
49. Kelly, E. L., and Fiske, D. W., *Am. Psychologist*, **5,** 395–406 (1950)
50. Klein, G. S., *Perception: An Approach to Personality*, 328–55 (Blake, R. R., & Ramsey, G. V., Ed., Ronald Press Co., New York, 442 pp., 1951)
51. Köhler, W., *The Mentality of Apes* (Kegan Paul, Trench, Trubner & Co., Ltd., London, 336 pp., 1931)
52. Köhler, W., and Wallach, H., *Proc. Am. Phil. Soc.*, **88,** 269–351 (1944)
53. Krech, D., *Psychol. Rev.*, **57,** 345–61 (1950)
54. Krech, D., *J. Personality*, **18,** 66–167 (1949)
55. Krech, D., *Psychol. Rev.*, **57,** 283–90 (1950)
56. Kretschmer, E., *Körperbau und Charakter* (Springer-Verlag, Berlin, 349 pp., 1951)
57. Kroh, O., *Psychol. Rundschau*, **1,** 26–36 (1949)
58. Lersch, P., *Psychol. Rundschau*, **1,** 17–25 (1949)
59. Luft, J., *J. Abnormal Social Psychol.*, **45,** 756–59 (1950)
60. MacCorquodale, K., and Meehl, P. E., *Psychol. Rev.*, **55,** 95–107 (1948)
61. McCurdy, H. G., *Psychol. Rev.*, **57,** 322–27 (1950)
62. McKeouwn, J. E., *Am. J. Sociol.*, **56,** 175–79 (1950)
63. McLeish, J., *Quarterly Bull.*, **2,** 47–52 (1951)
64. McNemar, Q., *J. Genetic Psychol.*, **42,** 70–99 (1933)
65. Martiny, M., *Essai de Biotypologie Humaine* (J. Peyronnet & Cie, Paris, 497 pp., 1951)
66. May, J., *J. Mental Sci.*, **97,** 466–67 (1951)
67. May, R., *The Meaning of Anxiety* (Ronald Press Co., New York, 376 pp., 1950)
68. Mensh, I. N., *Psychol. Bull.*, **47,** 475–92 (1950)
69. Milliken, J. R., and Standen, J., *J. Neurol. Neurosurg. Psychiat.*, **14,** 47–54 (1951)

70. Mowrer, O. H., *Learning Theory and Personality Dynamics* (Ronald Press Co., New York, 776 pp., 1950)
71. Newman, H. J., Freeman, F. N., and Holzinger, K. J., *Twins: A Study of Heredity and Environment* (Univ. Chicago Press, Chicago, 369 pp., 1937)
72. Nieden, M. Z., *J. Psychol.*, **31**, 91–95 (1951)
73. Pastore, N., *The Nature-Nurture Controversy* (King's Crown Press, New York, 213 pp., 1949)
74. Petrie, A., *J. Mental Sci.*, **95**, 449–55 (1949)
75. Petrie, A., *Brit. J. Med. Psychol.*, **22**, 200–7 (1949)
76. Petrie, A., *Proc. Roy. Soc. Med.*, **42**, 39–40 (1949)
77. Petrie, A., *Lancet*, **II**, 325–27 (1948)
78. Petrie, A., and Powell, M. B., *Lancet*, **I**, 363–65 (1950)
79. Rees, L., *J. Mental Sci.*, **96**, 619–32 (1950)
80. Rees, L., *J. Mental Sci.*, **96**, 426–34 (1950)
81. Reyburn, H. A., and Raath, M. J., *Brit. J. Psychol., Statist. Sect.*, **3**, 150–58 (1950)
82. Roff, M., *J. Psychol.*, **30**, 199–228 (1950)
83. Rohrer, J. H., and Sherif, M., *Social Psychology at the Crossroads* (Harper & Bros., New York, 437 pp., 1951)
84. Rosenzweig, S., *J. Abnormal Social Psychol.*, **45**, 507–9 (1950)
85. Schneider, L. I., *Psychol. Bull.*, **47**, 493–508 (1950)
86. Schreider, E., *Biotypologie*, **11**, 9–18 (1950)
87. Schwartz, L., *Die Neurosen und die dynamische Psychologie von Pierre Janet* (Benno Schwabe, Basel, 465 pp., 1951)
88. Sen, A., *Brit. J. Psychol., Statist. Sect.*, **3**, 21–39 (1950)
89. Sheldon, W. H., *Varieties of Delinquent Youth: An Introduction to Constitutional Psychiatry* (Harper & Bros., New York, 899 pp., 1949)
90. Sheldon, W. H., *The Varieties of Human Physique* (Harper & Bros., New York, 347 pp., 1940)
91. Sheldon, W. H., *The Varieties of Temperament* (Harper & Bros., New York, 519 pp., 1942)
92. Slater, E., *Recent Progress in Psychiatry*, 1–25 (Fleming, G. W. T. H., Ed., J. & A. Churchill, Ltd., 711 pp., London, 1950)
93. Stagner, R., *Psychol. Rev.*, **58**, 5–17 (1951)
94. Stephenson, W., *Character and Personality*, **4**, 17–24 (1935)
95. Stephenson, W., *Brit. J. Psychol.*, **26**, 344–61 (1936)
96. Stephenson, W., *J. Educ. Psychol.*, **27**, 358–67 (1936)
97. Stephenson, W., *Feelings and Emotions*, 552–70 (Reymert, M. L., Ed., McGraw-Hill Book Co., Inc., New York, 603 pp., 1950)
98. The O.S.S. Assessment Staff, *Assessment of Men* (Rinehart & Co., Inc., New York, 541 pp., 1948)
99. Thistlethwaite, D., *Psychol. Bul.*, **48**, 97–129 (1951)
100. Tizard, J., and O'Connor, N., *Am. J. Mental Deficiency*, **54**, 563–76 (1950)
101. Tizard, J., and O'Connor, N., *Am. J. Mental Deficiency*, **55**, 144–57 (1950)
102. Warren, W., *Brit. J. Delinquents*, **1**, 164–86 (1951)
103. Wittenborn, J. R., *J. Consulting Psychol.*, **14**, 261–67 (1950)
104. Wortis, J., *Soviet Psychiatry* (Williams & Wilkins Co., Baltimore, 314 pp., 1950)

XUM

SOCIAL PSYCHOLOGY AND GROUP PROCESSES[1]

By M. Brewster Smith

Department of Psychology, Vassar College, Poughkeepsie, New York

As an area of converging endeavor from a number of historically separate lines of development, social psychology has in recent years shown both centrifugal and centripetal trends. Centrifugally, its boundaries have become increasingly blurred as it has sunk deeper roots into general psychology, while at the same time it has tended to fuse with the psychology of personality and to share a widening area of common concern with sociology, anthropology, and the other social sciences. The centripetal trend toward a systematic organization of the field in its own right has been more faltering and controversial. Newcomb (86), a notable contributor to this theoretical development, has laid out the directions for one such integration of the field:

> If social psychology is to take as its special province the study of interaction, it must make a special place for those kinds of interaction which presuppose shared frames of reference, and by which shared frames of reference are acquired.

MacLeod (76), on the other hand, voices a common misgiving when he says that

> in speaking of social psychology as a separate discipline we may be doing violence to the facts, and we may even be impeding the progress of psychological theory.

While the past year leaves social psychology without consensus on its scope or organizing principles, research has made substantial progress in a number of limited but increasingly well-conceptualized areas.

Contributing to the rapid if rather chaotic growth of the field has been a sense of urgency that has led to generous support from the government and foundations for research aimed at a better understanding of social behavior. The Office of Naval Research, a major patron, has sponsored a farsighted program of basic research in human relations on which progress reports for the first five years are presented in a volume edited by Guetzkow (47). During the year, the trustees of the newly-created Ford Foundation (37) announced that as Area V of their projected program,

> The Ford Foundation will support activities designed to increase knowledge of factors which influence or determine human conduct, and to extend such knowledge for the maximum benefit of individuals and of society.

Among the activities specified as falling within this broad mandate are the scientific study of "the interaction of biological, interpersonal, and cultural influences in human behavior," of "values which affect the conduct of individuals," of "the processes of communication," and of "group organization,

[1] The survey of the literature to which this review pertains was completed about June, 1951.

administration, and leadership." The unprecedented support provided by these and similar programs offers the opportunity for further acceleration in the growth of social psychology. It will also, however, seriously tax the supply of qualified workers, while the high expectations underlying the investment of large funds lay upon the field a degree of responsibility that is perhaps a mixed blessing.

The trend toward obliterating the boundaries between social psychology and the other social science disciplines has been continued by publications extending psychological methods and conceptions into economics and political science. In an extensive review of the literature, Hayes (50) explores the historical role of psychological assumptions in economic theory and indicates the special contributions to be expected from a psychological approach. A major volume by Katona (61) weds empirical microeconomics with field theoretical psychology, marshalling current evidence, much of it gathered under his direction at the Survey Research Center, about psychological factors in consumer and business behavior and in economic fluctuations. Garceau (40) examines research trends in the social sciences for their bearing on the study of the political process, which he conceives in terms of overlapping interaction patterns along a continuum between the individual citizen and the strategic points of public policy making. In each of these instances, psychological techniques and conceptions have been brought to bear on problems of other social sciences restated in terms of the patterned behavior and interactions of individuals. To such collaborative ventures, social psychology seems at present to have more to offer in research methodology than it has in established and interconnected principles.

Since there is little agreement about the boundaries of social psychology, a residual working conception of the field has been employed here, excluding work conventionally embraced in other subdivisions of psychology or in the central territory of the other social sciences. Research using animal subjects has not been covered. In the selection imposed by space limitations, an attempt has been made to represent major theoretical and methodological developments in the principal topical areas of current activity. These have been grouped for convenience under the following headings: cognitive processes in social behavior, group dynamics, leadership and organizational behavior, the social framework of behavior and attitudes, culture and personality, ethnic prejudice and intergroup tension, and opinion and attitude methodology.

Cognitive Processes in Social Behavior

The cognitive processes have recently been the subject of active and still controversial work that has made a strong bid to integrate important facets of personality theory, social, and general psychology. Under the loose heading of social perception, it has become somewhat customary to group two related areas of investigation which, as MacLeod (76) has pointed out, need to be distinguished: studies of social factors in perception and studies of the perception of the social.

Social determinants of perceptual and cognitive processes.—The program of Bruner, Postman, and their collaborators has been extended beyond the connotation of social perception in two respects. They now are aiming at a formulation that embraces the cognitive processes generally, and rather than focussing on social determinants as a special case, they would include them without distinction among their instrumental (or directive) variables. As they put their views in two recent theoretical statements (16, 92), they propose an "hypothesis-information theory of cognition" in which the cycle of cognitive behavior in an active organism is seen as moving from hypothesis through information input to confirmation or nonconfirmation. Social and motivation factors are placed among the determinants of hypothesis strength and among the relevant conditions of hypothesis confirmation. While the effects of social determinants are brought out most clearly when the input of information is meager or unreliable, Bruner (16) holds that

> most complex perception, particularly in our social lives, is dependent upon the integration of information of a far less reliable kind than we normally provide in a tachistoscope at rapid exposure.

Essential to this theoretical framework is the assumption that instrumental processes of vigilance or defense may intervene between the registration of motivationally relevant information and the awareness and report of perception, a position challenged by Howes & Solomon (57). The existence of discriminatory processes without awareness, of a sort required by the Bruner & Postman theory, has been demonstrated by Lazarus & McCleary (71), who obtained differential conditioned galvanic skin responses to nonsense syllables at exposure speeds faster than the report threshold.

The hypothesis that symbolic value in a perceptual object leads of itself to perceptual accentuation, supported by Bruner & Postman (17) in an earlier study, has been challenged by Klein and his collaborators (69), who repeated the study with variations and found essentially negative results, though obtaining significant individual differences in perceptual behavior. Klein stresses the importance of stable individual differences in preferred perceptual attitudes that affect the perceiver's intentions when confronted with an object, and he questions in these terms the rationale of the accentuation hypothesis. Bruner (16) has recently conceded this point, remarking that

> what is theoretically wrong about most of these studies, both those which have and those which have not found perceptual accentuation, is that there is rarely stated a specific hypothesis about why the attribute being studied should be influenced by behavioral factors.

The present positions of both groups of investigators appear rather to complement than to conflict with one another.

While the work just reviewed bears on underlying processes by which determinants of social origin may affect perception and cognition, characteristically investigated under conditions of relatively weak motivation and minimal information, Asch (7) has experimented more directly with specifically social influences, with the further difference that his settings

involve unambiguous stimulus conditions and maximum social pressures. In his typical situation, an experimental subject in a group with seven accomplice subjects makes a series of judgments with different sets of lines, matching the length of a standard line with one of three supraliminally different comparison lines. By prearrangement, the accomplices announce objectively incorrect judgments in a majority of cases. The subject's errors on critical trials are recorded and his understanding of the situation is explored in a postexperimental interview.

Under these conditions, Asch has found in groups of college men that about one-third of the critical estimates erred in the direction of the majority, with extreme individual differences in reaction. Among subjects showing independence of the majority judgment, he distinguishes confident, withdrawn, and anxious types; while among the "yielders," the greater number showed a distortion in judgment but not in perception, some modified their reports in spite of correct judgments, and a few appeared from all indications to have had their actual perceptions distorted.

This basic situation lends itself to endless variations, of which Asch reports results for a number. As long as the subject is a minority of one, for example, a unanimous majority of three was as effective as one of eight or sixteen, but the addition of one accomplice giving correct judgments lowered the proportion of pro-majority errors to about 5 per cent. The latter finding would seem relevant to the role of censorship in totalitarian propaganda, as well as to the social function of a vocal minority. Asch's results show that social influence on perception and judgment is not limited to unstructured situations. The relation of the individual differences described by him to other personality variables is a promising area for future exploration which may tie his findings to those of Klein (69) and of the investigations stemming from the University of California studies of prejudice (2).

The bearing of stimulus structure on cognitive distortion has also concerned Thistlethwaite (109) in his study of the effect of ethnocentric attitudes on distortion of reasoning. He develops a conception of structure as having degree and direction, measurable in his experimental setting by the proportion of valid or invalid responses given by a particular subject to neutral instances of a logical form. The greatest degree of distortion, he finds after appropriate statistical analysis, occurred not with the most "ambiguous" structures (those for which a subject's valid and invalid responses were evenly divided on neutral instances) but with those structures most consonant with the subject's relevant beliefs and attitudes.

Much of the recent research on social factors in cognition owes its inspiration to Bartlett's classic work, *Remembering* (11). Long out of print, it is again available. Allport & Postman's studies of changes in rumor content during serial transmission (4), a direct outgrowth of Bartlett's work, have recently been repeated by Higham (54), using matter-of-fact stories and pictures. His results, analyzed in terms of levelling, sharpening, and assimilation, generally confirm those of Bartlett and of Allport & Postman.

Cognition of the social.—Research on the perception of social facts, in-

cluding the attributes of other persons, has lagged behind the study of social determinants of perception. Apart from the largely separate tradition of research on social stereotypes to be reviewed later, two sorts of methods have characteristically been used in these studies.

According to the method introduced by Asch (6), subjects are asked to give intuitive personality sketches of fictitious persons on the basis of a list of descriptive adjectives, one of which is varied systematically from list to list. Interest centers on the effect of different particular attributes on total impressions of personality. Haire & Grunes (49), in an ingenious adaptation of this technique for a study of industrial relations, gave subjects lists of adjectives descriptive of a factory worker, the lists being identical with the exception of including the word "intelligent" for some subjects and not for others. The incidental findings demonstrated that defensive tactics similar to those identified in other studies of cognitive distortion are used in the protection of personal stereotypes. "Intelligent" did not fit the conception of a factory worker held by most of their college student subjects. In the personality sketches drawn from the lists including this adjective, four patterns of response to its presence were detected: denial, encapsulation and distortion, real change in organization, and maintenance of the stereotype together with recognition of the incongruity of the adjective.

Asch's finding (6) that the warm-cold variable has an organizing influence on impressions of personality was confirmed in a study by Kelley (65) of first impressions of real persons, with essentially similar results. In addition, Kelley showed that the experimentally introduced differences in expectations influenced the subjects' behavior, expectation of warmth leading to more favorable interaction.

The second group of methods has in common the measurement of the accuracy with which subjects are able to estimate the responses of other individuals or groups to opinion or personality items. This approach is followed by Kitt & Gleicher (68) in their analysis of estimates of vote intention in the Elmira study of the 1948 presidential election. Their data indicate that people tend to err in the direction of their own vote intention when asked to estimate prevalent vote intention in various social groups. Moreover, the more strongly a person feels about his own party choice and the shorter his social distance from the group whose vote is to be estimated, the greater the degree of distortion. The over-all bias in social perception revealed in these findings is doubtless the gross product of such analytically distinguishable processes as selective association, selective perception, selective recall, and sheer projection, all of which might be expected to lead to effects in the obtained direction.

It has often been assumed, plausibly enough, that accuracy of social perception is a factor in effective social relationships. Conflicting data bearing on this assumption have recently been reported. Gage & Succi (39) have found that teachers who estimated student opinion on school affairs more accurately tended to receive higher ratings from their pupils, while Hites & Campbell (55) report that elected leaders, appointed leaders, and nonleaders

in eight college fraternities did not differ in their ability to estimate fraternity opinion, even on matters of daily concern in the functioning of the groups. In view of the reasonableness of the hypothesis, further studies are clearly needed in which such group characteristics as homogeneity, adequacy of communication, and hierarchical structure are systematically varied.

While the program of social phenomenology recommended by MacLeod (76) has mainly received lip service, Barker & Wright (10) in their related studies of psychological ecology have set out to catalogue the environmental settings in which social behavior occurs. Working with full anecdotal records of a child's day, they classify the relevant environmental facts under three broad headings: (*a*) the nonpsychological milieu which provides the physical and social raw materials of the psychological situation; (*b*) behavior-settings and behavior-objects, discriminable parts of the nonpsychological milieu generally perceived as necessary or appropriate for the transaction of some particular behavior; and (*c*) the psychological habitat, equivalent to the life space or relevant psychological context of behavior. From a statistical analysis of behavior-situation episodes, they anticipate that lawful regularities will emerge.

The Study of Group Dynamics

One of the milestones in the development of social psychology was the still recent achievement of Lewin and his students when they brought complex group processes under experimental scrutiny. Lewin's application of his field theoretical approach to problems in this area and others is now readily accessible in a volume reprinting a selection of his theoretical papers (73). Since Lewin's death, studies of processes in small groups have burgeoned rapidly, not only among his followers in the Research Center for Group Dynamics at the University of Michigan, but also among workers of diverse theoretical persuasions, and have shown a steady trend toward greater theoretical and methodological sophistication.

Communication and interpersonal influence in small groups.—The results of an impressive program of experimentation centering on an explicit theory of social communication have been published during the past year by Festinger and his associates at the Research Center for Group Dynamics (8, 9, 32, 33, 66, 98, 108). In Volume 2 of this series, Katz (62) has already given a detailed review of the program, which is therefore treated in more general terms here. Three problem areas have been the subject of investigation: the transmission of rumor and information in groups, restraints against communication in hierarchies, and communication as the exertion of influence. Taken as a whole, the series of studies is an example, all too rare in the literature, of a program in which field study has led directly to controlled experiment, and successive experiments, designed to test an increasingly complex set of hypotheses, have interlocked to yield a growing body of generally positive and systematically intelligible results.

The chief dependent variables in the recently reported experiments have

been frequency of interpersonal or intergroup communication, degree of social influence on individual productions or opinions, and, less frequently, sociometric choice or rejection. Among the independent variables may be noted the following: direct pressure toward uniformity; personal attraction to other group members, task motivation, and group prestige as sources of group cohesiveness (shown to be an intervening source of pressures toward uniformity); individual deviance or conformity with respect to the group norm; homogeneity or heterogeneity of perceived group composition; relevance of issue to the purposes of the group; high or low status of the group in a hierarchy; and perceived potential mobility from high or low hierarchical status.

The experimenters have shown ingenuity in devising situations that permit the manipulation, control, and measurement of the relevant variables with a minimum of artificiality from the standpoint of the subject. The adequacy with which the experimental conditions embody the intended theoretical variables must nevertheless remain open to some debate, the more so because much hinges on the effects of intervening variables, which can be varied experimentally only by the use of indirect means that may have additional unintended psychological consequences. To some degreee the program has employed the safeguard of using different methods in interlocking experiments to check previous results. As the body of experimental findings grows, one's confidence in the co-ordination of theoretical to empirical concepts should increase.

It is of some interest to note that these studies, while conceived in the tradition of field theory, do not employ or depend on Lewinian topological representation. Concerned with a limited theory closely linked to experimental fact, they contribute a block of knowledge that can conceivably be integrated in any of a variety of larger structures.

A different approach to interpersonal influence is shown in the study of Polansky, Lippitt & Redl (91) on behavioral contagion in groups of disturbed children at a therapeutic summer camp. They were concerned with factors leading particular children to initiate activities that are picked up by others in a group or to allow themselves to be influenced by others in their own behavior. Data from systematic observation, quasi-sociometric techniques, and counsellors' ratings showed that high group prestige position, implying greater security for spontaneous action, was associated both with relatively frequent attempts to influence others and with relative openness to behavioral contagion. Only in stressful situations were the low in prestige more likely to initiate behavioral contagion. Their greater impulsiveness led them to give early vent to the usually inhibited reactions toward which the group as a whole was then predisposed. This study brings under systematic investigation phenomena of group behavior that have remained the province of speculation and anecdotal description since the time of Tarde and LeBon.

The foregoing studies have been interested in the patterns of informal

communication and influence that develop under conditions in which the possibilities for communication are relatively unrestricted. Leavitt (72), on the other hand, reports on work done under the direction of Bavelas, in which the possible channels of interpersonal communication are narrowly limited and the effects of different prearranged communication patterns on efficiency of communication and emergent group structure are the focus of interest. The situations studied, necessarily more artificial than those employed in the study of communication under freer conditions, acquire importance from their connection with the mathematical model for group communication networks previously developed by Bavelas (12). Leavitt found that a measure of centrality, a function of the total of the shortest number of steps from a position in a network to every other position, was related to important behavioral differences. In networks in which centrality was evenly distributed among the various positions, for example, no leader was likely to appear and group organization was slow to develop. While activity level and satisfaction were likely to be high, low efficiency was reflected in a high error record. Centrality is viewed as affecting behavior by the limits it imposes on independent action.

Research on the conference process.—A more pragmatic orientation underlies the University of Michigan Conference Research Project, described with preliminary results in a summary statement by Marquis, Guetzkow & Heyns (77) and already reviewed in some detail by Katz (62). In extensive field and laboratory studies, they have sought to identify the factors underlying the effective and ineffective functioning of decision-making conferences. Such a program differs from one like Festinger's (32) in three important respects: (*a*) concerned with evaluating results rather than describing process, it is faced with the problem of devising appropriate criteria of effectiveness; (*b*) the search for relationships between the criteria and potentially influential factors has tended to be exploratory, since few theoretical guide-lines were available; and (*c*) since the basic measures pertain to entire groups rather than to their component individuals, each study has required the observation of a large number of groups in order to secure statistically stable results.

The ambitious scope of this program may be illustrated by a recently conducted field study of 72 conference groups in business, industry, and government (77). The criteria employed—satisfaction after meeting, productivity (in terms of the proportion of agenda items decided), and extent of residual disagreement—were found to be relatively uncorrelated with one another. Satisfaction correlated positively with cohesiveness and procedural structuring, negatively with self-oriented need behavior; productivity correlated positively with urgency of the problem and power to deal with it, negatively with self-oriented need; residual disagreement was associated with disagreement as to goals, self-oriented need, and inadequate power. Although considerable methodological advances have been made by the program in the development of reliable observational techniques, the needs for adequate theory and satisfactory criteria remain serious obstacles.

The search for group dimensions.—An evident need in all group studies, whether the emphasis be on internal group process, objective achievement, or effects of leadership, is for adequate variables to describe the attributes of group structure and functioning. Such variables may be defined *ad hoc* in connection with the special purposes of a study or arrived at on theoretical grounds. A standard set of dimensions, however, would have manifest advantages in permitting cumulative research. Work on this problem has been going on for some time from two divergent approaches.

Hemphill & Westie (53), as part of the Ohio State Leadership Studies, have developed Likert-type scales to measure each of the 14 group dimensions which Hemphill (51) had previously defined. While the scales were constructed by the use of judges and conventional item-analysis procedures and have satisfactory reliability for most purposes, the dimensions that they measure were arrived at somewhat arbitrarily. The comprehensiveness and explicit definition of the dimensions, nevertheless, should make the scales widely useful in research designs requiring the description of groups on the basis of questionnaires administered to their members.

An arduous attempt to arrive at more fundamental dimensions by applying factor analysis to measurements on actually observed groups is being made by Cattell (22), who is highly critical of research that has gone on to study presumed determinants of group functioning without first clarifying the basic descriptive variables by this method. His current work, based on the intercorrelation of some 150 variables for 80 10-man groups, gives promise of important results. One may nevertheless question whether the dimensions of "syntality" that emerge will serve all descriptive and theoretical purposes and supplant variables defined in terms of their relevance to particular theoretical or practical contexts.

Leadership and Organizational Behavior

Much research on leadership, conducted in small groups for the sake of the experimental manipulations possible, might properly be grouped with the studies of group dynamics in the preceding section. Leadership is, nonetheless, pre-eminently a problem of larger, more complex organizations, and when it has been studied in small group settings, the explicit or implicit intent has often been to extrapolate the conclusions to organizations or even to society at large. One of the healthy trends detectable in recent work is toward increasing skepticism about the legitimacy of such extrapolation. The realization is growing that isolated face-to-face groups, informal groups embedded in organizational contexts, and larger organized units are likely to present different problems. While one may hope that leadership in all these settings may eventually be embraced in a general theory of interpersonal relations, the path of caution lies in carrying out studies in the kind of setting to which it is desired that the conclusions will apply.

Whether studied in small groups or in large organizations, leadership no longer seems a simple phenomenon. While the implicit traditional view of

leadership as a personality trait has given way to conceptions more faithful to the leader-follower relationship, the simplistic situational position that has attained some currency appears equally untenable. Bell & French (13), for example, found striking consistency of individual leadership position in discussion groups composed of all possible five-man combinations from a pool of 25 subjects. They conclude that

> varying group membership in this situation accounts for at most a relatively small portion of the variance in leadership status.

Not only are the group and individual factors that may affect the nature and effectiveness of leadership numerous and complexly interrelated, but as leadership studies have taken an increasingly prominent place in the literature, it has become evident that there is little uniformity in the operational meaning given to the central concepts. The Ohio Leadership Studies have made noteworthy contributions toward bringing a degree of order into this rather chaotic situation. Morris & Seeman (82) present a useful schema, developed for this program, for the variables, criteria, and sources of data that need to be considered in leadership research but are often neglected by default. Their array of alternative definitions, transcribed below, highlights some of the initial decisions that must be taken in research in this area.

Definitions of the Leader:

(*a*) Individual in a given office; in a standardized position of high influence potential. (*b*) Individual who exercises positive influence acts upon others. (*c*) Individual who exercises more, or more important, positive influence acts than any other member in the group. (*d*) Individual named by the group as leader. (*e*) Individual who exercises most influence in goal setting and goal achievement.

Definitions of Leader Behavior:

(*a*) Behavior involved in the execution of a given position. (*b*) All the behavior of the individual selected as leader. (*c*) Any positive influence act. (*d*) Behavior of any individual that makes a difference in the behavior or characteristics of the group. (*e*) Behavior of an individual when he is directing the activities of a group.

The concept of democratic leadership.—The earlier experimental work on leadership depended heavily on small group settings and was strongly influenced by the classic studies of democratic and autocratic leadership by Lewin, Lippitt & White (74). The continuing influence of these studies, stimulating as they were to the development of group dynamics, has not been entirely advantageous. Under the value-laden headings of democracy and autocracy they somewhat arbitrarily packaged a number of potentially distinct variables of leader role, the several consequences of which have only begun to be disentangled. They also established the small group as the paradigm for evaluating the consequences of different styles of leadership. Small group studies have in general found that favorable consequences accompany permissive, member-centered modes of leadership which encourage a maximum of participation.

The general applicability of these conclusions has recently been qualified in several respects. Hemphill (52), for example, concludes that the size of the group makes a critical difference in the behavior of leaders who are considered superior by group members. Comparing questionnaire descriptions of superior leaders of groups larger or smaller than 30, he found a consistent pattern of significant differences. Inspection of these differences led him to formulate the following hypotheses: (*a*) As the group becomes larger, demands upon the leader's role become greater and more numerous; and (*b*) as the group increases in size, tolerance for leader-centered direction of group activities becomes greater. The heavier demands on leadership in larger groups, he supposes, result in a tendency for it to take on a greater degree of impersonal direction connected with firmness and impartiality in the enforcement of rules and regulations.

Besides the factor of group size, the prevailing attitudes of group members may influence the effectiveness of different styles of leadership. In a pilot study among teachers in four school systems, Seeman (99) administered a scale pertaining to the behavior of the "ideal superintendent," and a scale measuring status attitudes, ranging from commitment in favor of status differences to opposition to them. The results showed a considerable demand for directive leadership, with the "highs" in status attitudes exhibiting the stronger pro-leader ideology. While preference for a particular style of leadership is of course no guarantee of the effectiveness of that style in practice, the results are plausible. Seeman draws from them the theoretical implication that problems of leadership should not be artificially separated from the more general ones of power, influence, and social status.

The follower's orientation to authority as a potential influence on leadership has also been the subject of intensive investigation by Sanford (97) and his associates in the Institute for Research in Human Relations, in a sampling interview study of 963 Philadelphians. Taking their start from a different theoretical context, they arrive at conclusions that converge to a considerable extent with Seeman's. The University of California studies of authoritarianism and prejudice by Adorno *et al.* (2) provide the underlying rationale for their investigation. Authoritarianism according to these studies is not a mere congeries of attitudes selected for their relevance to democratic values, but a psychologically coherent personality syndrome arising from a common dynamic basis. The Philadelphia group have been concerned with further delineating this cluster of authoritarian attitudes, tracing its implications for orientation to leadership in a variety of settings and mapping its distribution among various demographic groups.

Their basic instrument is an eight-item authoritarianism-equalitarianism scale largely adapted from the California F-scale (2). Odd-even reliability of .48 and repeat reliability of .78 are reported. The items are these:

(*a*) Human nature being what it is, there must always be war and conflict. (*b*) The most important thing a child should learn is obedience to his parents. (*c*) A few strong leaders could make this country better than all the laws and talk. (*d*) Most people who don't get ahead just don't have enough will power. (*e*) Husbands

should help their wives with the dishes and care for the children. (Reverse scored.) (*f*) Women should stay out of politics. (*g*) People sometimes say that an insult to your honor should not be forgotten. (*i*) People can be trusted. (Reverse scored.)

The scale was embedded in an interview exploring preferred relations to leaders by a variety of direct and indirect techniques, some of them original and ingenious. Authoritarian-scoring followers, Sanford concludes from his data, tend to be concerned with status and power in leaders, and while they prefer strongly directive leadership their underlying ambivalence leaves them ready to reject the leader who shows signs of weakness or nonconformity. Personalizing the leader rather than emphasizing his social functions, they nevertheless look to him for material support rather than warmth and approval. In their concern with their individual relationship with the leader, they pay little attention to the welfare of their fellow followers and are relatively reluctant to involve themselves in group activities or to accept responsibility. Equalitarians on the scale show contrasting features. Many of the above findings were only tentatively established in this exploratory investigation. Now that the feasibility of measuring such deep-seated dispositions in the population at large has been demonstrated, the way is open for studying their relation to observed patterns of organizational leadership.

The categories of democratic and autocratic leadership that formerly dominated leadership research are thus undergoing radical revision. A shift in emphasis from the small group to the organization has in effect led to a politically more sophisticated grasp of what democratic leadership implies. Categories derived from prior value-assumptions are giving way to similar but more psychological distinctions based on coherent functional relations.

Organizational studies.—Until recently, most of the significant empirical work on leadership, morale, and productivity in complex social organizations was being done by industrial sociologists and applied anthropologists rather than by psychologists. The assumptions, concepts, and methods underlying this tradition, which characteristically based its conclusions on naturalistic participant observation rather than on quantitative data, have recently been reviewed by Arensberg (5). A book by Homans (56) in this line of development attempts to formulate a minimal conceptual scheme for behavior in groups and organizations conceived as social systems. From the primitive concepts of activity, interaction, sentiment, and norm, he develops hypotheses in terms of which he discusses a series of group case studies. While his scheme has formal elegance and his case discussions contain provocative insights, psychologists will probably find his basic conceptual distinctions too gross for ready adaptation to quantitative research.

Progress reports have emerged this year from two major programs of quantitative research on organizational behavior, both under the direction of psychologists but paying more than lip service to the facts of social structure. Shartle (101) describes the Ohio Leadership Studies, which have been studying relations between superiors and subordinates in a variety of industrial, educational, and military settings. Among the measures that

have been employed in studying the leader are ones of time spent on various activities, the degree of responsibility, authority, and delegation, and the modes of leadership behavior. The previously discussed investigations of group dimensions (53) and of the influence of status attitudes (99) form part of this series, supplementing attention to the individual leader with attention to the group or staff and to the culture in which the individual and the staff are found. Within the framework of large organizations, leader-follower relations are complicated by the presence of hierarchical levels of leadership which must be studied as a whole if misleading results are to be avoided. Shartle states, for example:

> We find that those leaders who delegate more authority tend to have assistants who in turn delegate more to their assistants . . . . If one assumes, and there is evidence for it, that the top person in the chain has the most influence in establishing and maintaining the degree of delegation down the line, it is obviously very important to know the behavior of the top person.

The Michigan Survey Research Center has been engaged in a related program aimed at delineating the conditions of organization which make for a high level of group productivity and a high level of satisfaction among group members. Described by Katz, Kahn, Jacobson, Morse & Campbell (64), the program and its findings are also reviewed by Katz (62) in Volume 2 of the *Annual Review of Psychology*.

Like earlier students of morale under military conditions (105), the Michigan group has found it necessary to break away from a blanket concept of morale. Factor analysis of some 70 satisfaction items in one study in heavy industry isolated four factors: (*a*) satisfaction with immediate supervision, (*b*) satisfaction with the job itself, (*c*) satisfaction with the organization as a system, and (*d*) an indirect satisfaction measure related to wages and opportunities for mobility. Their studies of the impact of supervisory practices on morale and productivity have carried them into widely varying settings, including an insurance company, a public utility, heavy industry, unskilled railroad labor, and a government office.

Of general consequence is their finding from correlational studies, that contrary to commonly accepted principles of administration and management, high producing groups tend to be ones characterized by delegated authority and loose but person-centered supervision. More conclusive evidence on this score should be forthcoming from the long-term field experiment that they are now conducting in an insurance company to test the influence on productivity and morale of shifting the locus of supervisory control and regulation upward or downward in the line of authority. The difficulties and mischances described in the field administration of this experiment are ample reason why such a clearly indicated step is so rare in the literature.

The Social Framework of Attitudes and Behavior

One of the important theoretical trends toward integration in social psychology has its focus in the co-ordination of sociological concepts—role,

reference group, and class are among the more important of them—to psychological analysis of individual behavior and predispositions. Without such concepts, traditional social psychology remained too atomistic to be of much use to the other social sciences or, dissatisfied with individualism, made the magic leap into theories of the group mind. Sociological theory in turn stood to benefit from a closer bond to the facts and established research methods of psychology. This trend toward integration, as illustrated in the work of Newcomb (86), is still too recent for research to have caught up with the theoretical vanguard. Nevertheless, some important work has been reported during the past year.

Research on roles.—While the concept of social role has played an incidental though indispensable part in much of the work on group dynamics and organizational behavior, relatively little empirical research has been devoted to the systematic clarification of the concept in its psychological implications. In the research that has been done, situations of role conflict typically have been selected for study not only because an understanding of how such conflicts are resolved is essential for the prediction of behavior, but also because of their presumed relevance to personality disorder.

Stouffer & Toby (107), in the second of two papers applying questionnaire methods to the study of roles, demonstrate consistent individual differences in the predisposition to give precedence to institutionalized social obligations over the "particularistic" obligations of friendship, when conflicts of this character are presented for choice in hypothetical situations. They see in such results the basis for an empirical link between role and personality theory. To show consistent and unidimensional individual differences they employ the newer scaling methods of Guttman and Lazarsfeld (106), to be discussed later.

Role conflict as a factor in marital happiness has been studied by Ort (87) through systematic individual interviews with student couples. He gathered four types of information: role expectations for self, role played by self, role expectations for partner, and role played by partner. While his analysis of discrepancies between actual and expected roles supports his hypothesis that role conflict is negatively related to happiness, he unfortunately fails to exploit some of the more interesting possibilities in his data. By confining his analysis to subjectively perceived role discrepancies within the report of each partner, he neglects potential evidence about objective conflicts between the members of a marital pair in regard to role expectation and role behavior.

Reference group theory.—The concept of reference group, introduced by Hyman (58) in his pioneering study of the psychology of status, has been seized upon avidly by psychologists and sociologists. As Merton observes (80), however, little systematic attention has been paid to the important question:

> Under which conditions are associates within one's own groups taken as a frame of reference for self-evaluation and attitude-formation, and under which conditions do out-groups or non-membership groups provide the significant frame of reference?

Since, as he remarks, reference groups are potentially almost innumerable, principles accounting for a person's selecting a particular reference group are needed if the concept is to do more than provide a facile source of *post hoc* explanations. In a long essay that places the concept in a well-developed theoretical context, Merton (80) makes intensive use of data from *The American Soldier* (105) to achieve a remarkable degree of clarification in the researchable issues.

Other lines of psychological research may be brought into this context to mutual advantage. The problem of cross-pressures from identifications with both union and management, to choose an example from the Survey Research Center's Human Relations Program (64), is essentially a matter of reference groups. Also clearly related is the body of work on scales of judgment, the relevance of which to social psychology has recently been discussed by Volkman (110).

Psychological studies of social class.—Further clarification of the relationship between membership groups and reference groups should help to resolve some of the controversy that prevails concerning the criteria and consequences of social class membership. Centers (24) reports on the class attitudes of junior high school children, another of a series of studies employing sampling-survey and questionnaire methods to investigate the psychological accompaniments of class position. His findings that prolabor and collectivist attitudes are substantially correlated with parental occupational status and with subjective class identification agree with his earlier results. The Warner tradition is represented by Phillips (90), who describes the construction and validation of attitude scales for a similar age group to measure affiliation with each of the three major classses. He mentions, unfortunately without the supporting data, the provocative result that subjects from each of the three objectively defined social classes who deviate from their expected position on the attitude scales score lower on an intelligence test and on a measure of personal adjustment.

Culture and Personality

Klineberg (70) has provided a much needed critical guide to methods and results of research on the interrelations of personality and nationality. The year's crop of studies in this area, in which speculative essays, often brilliant, still outweigh the more solid evidence, bears out the timeliness of his skeptical yet positive evaluation of the field. He concludes:

> The fact that such a large variety of techniques have been distinguished in this field of study in itself indicates that no one technique has as yet been judged completely satisfactory. The problem of "national character" or of personality in relation to nationality is exceedingly complex. As a matter of fact, there have been frequent denials of the existence of national differences in personality . . . . It seems more probable . . . that although [such factors as economic level and degree of industrial development] must be taken into consideration at every step in the analysis, certain differences will still emerge in the behavior and attitudes of people of different nations . . . . The different techniques that have been distinguished all have something to be said in their favor; they all suffer from the same defect, namely, that their

validity has never been fully established. At the present time there seems to be only one adequate method of determining the validity of these techniques, and that is by a combined approach which uses several of them on the same national group.

The Columbia University Research in Contemporary Cultures, described by Mead (79), is the most ambitious program in which psychoanalytic assumptions about personality development have been applied to the interpretation of cultural integration as reflected in character structure. Seven cultures, Great Russian, Czech, Polish, Eastern-European-Jewish, French, Syrian, and Chinese, have been studied, for the most part through the use of informants and materials available in the United States. Of the studies coming from this program, Gorer's analysis of the Great Russians (43) has attracted the most attention and critical discussion. His hypothesis attributing great importance to the practice of tight and total swaddling as a way in which implicit cultural attitudes are communicated to the Russian infant and as the source of a number of hypothetical Russian character traits seems plausible to some and improbable to others; unfortunately the kind of systematic evidence that would be necessary for a convincing test is not presented.

More documentation is furnished in the latest of Kardiner's investigations of basic personality structure. Applying his conceptual approach to the American Negro as an oppressed variant of American culture, Kardiner & Ovesey (60) present a unique and valuable series of 25 psychoanalytic case studies. Read in the light of the case descriptions, the term "basic personality structure" seems to this reviewer partly misleading. Running through all the cases are nuclear conflicts centering around low self-esteem and aggression; but the strategies by which these conflicts are faced, mollified, or evaded vary remarkably from case to case. The analysis and conceptualization of the case materials are unsystematic, while test findings from the Rorschach and the Thematic Apperception Test (TAT) are not presented in a way that adds appreciably to the report.

French, English, and American movies, as representative cultural products, are subjected to psychological analysis by Wolfenstein & Leites (113) in a largely qualitative though systematic study. Among the themes they find distinctive of American movies is the "good-bad girl"—"bad" in initial aspect but always "good" beneath the appearances; such themes they interpret as collective fantasies revealing culturally prevalent dispositions.

The rigorous testing of hypotheses about culture-personality relationships logically requires cross-cultural research in which each culture provides but a single case in a sample to which normal statistical techniques may be applied. For such purposes, the Human Relations Area Files (formerly, Cross-Cultural Survey) (83), recently established at Yale and several other participating universities, make available on a large number of cultures source materials that would otherwise remain virtually inaccessible. While the original reports on which the files are based have serious limitations for psychological use, Whiting & Child, in a still unpublished study described by McClelland (78), have been able to employ the files to test several in-

XUM

teresting hypotheses drawn from psychoanalysis and Hullian learning theory.

Cattell (21) has applied the techniques of factor analysis to an even more fundamental problem: that of determining the dimensions of variation among existing nations and the major distinguishable culture patterns into which nations may be grouped. Working with a wide variety of data for 69 countries, he arrives, after staggering computations, at results that in a number of respects agree with common sense. Were the basic indices for his variables more inspiring of confidence, his ingenious methods would be highly promising; as matters stand, however, the structure seems to this reviewer too shaky to give much weight to his conclusion, for example, that race is an important influence in determining the similarity of culture patterns.

While there are no short cuts for evading the methodical work that needs doing if culture and personality studies are to earn scientific respectability, there is still room for insightful speculation. Two recent essays in this vein are the books by Erikson (29) and Riesman (93). Drawing on his experience as a psychoanalyst and some contact with two American Indian cultures, Erikson offers an account of the interplay between sociocultural milieu and personality development in the various stages of infantile sexuality and ego growth. One may turn to him for a version of psychoanalytic theory, still quite orthodox, grown sophisticated through acquaintance with modern anthropology. Riesman, on the other hand, in the most recent attempt to describe and interpret the American character, leans heavily on Fromm's characterology (38). The ramifications that he traces in a supposed shift from "inner-directed" to "other-directed" orientation, though gratuitously attached to speculations about the population cycle, contain many original insights that more systematic studies should not neglect.

Ethnic Prejudice and Intergroup Tension

A student of the sociology of knowledge might speculate that the persisting mobilization of psychological interest in research on prejudice represents a working out of social guilt in a relatively safe and uncontroversial area. Added theoretically-oriented impetus has come from the University of California studies (2) relating ethnocentrism to fundamental personality variables. World events have brought to the fore concern with factors in international as well as intergroup tensions, leading to research employing similar methods and concepts.

General considerations.—Discussion about the sources of ethnic prejudice has traditionally veered between emphasis on individual psychological causation and stress on sociocultural factors. Most informed treatments of the problem recognize the complementary nature of these analytic approaches, though they may disagree about the relative weights to be assigned to the different factors or about their relevance to the strategy of social action. A synoptic view of theory and research in prejudice emphasizing multiple levels of approach is presented by Allport (3) in his Kurt Lewin Memorial Address.

Defining prejudice as "negative attitudes toward human beings that are held because of their membership (or supposed membership) in a certain group," he distinguishes six levels in the study of prejudice, each with its essential contribution: approach via the stimulus object, the phenomenological approach, approach via personality dynamics and structure, the situational approach, the sociocultural approach, and the historical approach. The whole truth, it still seems necessary to point out, in case the implication of Allport's eclecticism is misunderstood, can hardly be embraced in one research program, still less in a single project. Research in this field will continue to be open to the essentially irrelevant and unjust charge of one-sidedness. Such criticism can be pertinent to attempts at theoretical integration; it has no bearing on the worth of particular contributions to the total structure.

However complex its determinants, prejudice is by definition a phenomenon at the individual level. Serious difficulties arise, however, when intergroup tension and conflict is treated as essentially a problem of individual prejudice and hostility. Even at the level of overt conflict between face-to-face groups, emergent factors of norms and group structure complicate the problem, as Sherif (103) has sought to demonstrate in a case study of group formation and conflict in a boys' camp. When the sources of conflict between nation states are in question, the trap of inappropriate psychologism is all too attractive, particularly since it is so much easier to study individual attitudes than it is to take systematic account of the broader social matrix.

This source of difficulty more than any other seems to bedevil the participants (Marxist and western; psychologists, psychoanalysts, and sociologists) in the fascinating international symposium sponsored by UNESCO on *Tensions That Cause Wars* (18). Horkheimer (18, p. 137), elaborating in this regard on a distinction between direct and mediated contacts among people, puts the matter thus:

> We should differentiate between the causes of tension which are "in the minds" of people, largely concomitant with *direct* human relationships, and those which operate over their heads, as it were, anonymously, as a consequence of the *absence* of immediate relations or . . . as a result of the reification of human relations.

Most of the psychological research on international tensions has not made this distinction. Its direct contribution is to the understanding of prejudice, ethnocentrism, and personal hostility; only with much qualification is it pertinent to the problems of international conflict.

Stereotypes and group preference.—The investigation of stereotypes of ethnic and national groups has continued to be an active field. While most research in this area is lacking in theoretical overtones, it makes a closer approach to social phenomenology than any other current work. A useful review of methods and results, with special reference to national stereotypes, is again provided by Klineberg (70). Postman (92) has linked an interpretation of stereotypes to his and Bruner's "hypothesis-information" theory of cognition, proposing that they be considered as

> strong monopolistic hypotheses about classes of objects and people which operate

without much competition from alternative hypotheses. As monopolistic hypotheses, they are confirmed by a small or indeed minimal amount of appropriate information.

Illustrative of several follow-up investigations that have tended to find a decrease in explicit ethnic and national prejudice among American student populations is one by Gilbert (42), who repeats, also on a Princeton sample, the widely cited study of Katz & Braly (63). He concludes that the present generation of college students is more reluctant than that of 18 years ago to make stereotyped generalizations. The generalizations that they do make "tend to be based more on cultural and historical realities and less on fictitious caricatures or on the prejudices of their parents." Most of the stereotypes depicted by Katz & Braly seem to have faded markedly, though many of the same adjectives are still prominent in the various descriptions.

An English study by James & Tenen (59) explores changing adolescent stereotypes of a variety of racial and national groups in a single series of free interviews. In order of favorableness of conception the groups were: American, Negro, (East) Indian, German, Italian, and Chinese, with the Japanese far at the bottom. The interview material, quoted extensively, indicates the importance of personal contact, often with soldiers or prisoners of war, as a factor in change of stereotype. When unfavorable stereotypes persisted, they appear to have done so by limiting contact and prejudicing such contacts as were made. Most of the children's statements about other peoples, they report, were confined to asserting their possession of disturbing or reassuring characteristics or of ones promising enjoyable personal relations. They found their respondents quite ready to generalize from individuals to peoples.

Morlan (81) provides data to attack the prevalent stereotype that attributes a distinctive body odor to Negroes. In a well-controlled experiment that is not likely to be repeated, he found that his mostly Northern subjects could not distinguish Negroes and whites by smell, regardless of the subjective certainty of their judgments.

Contributing to the study of national preferences is an inventory devised by Grace (45). He constructed items on the model of the Kuder Preference Record using names of countries, regions, rivers, and cities to measure preferences among nine geographical-cultural groupings, including a fictitious one. Dimensions of preference resulting from a factor analysis are reported, the first of four factors apparently referring to Oriental versus Western *Weltanschauung*. Grace makes much of the fact that the imaginary culture often appears in correlational groupings with Anglo-American culture. It is no more imaginary, however, than the fictitious names of rivers, cities, etc., with which he chooses to represent it; these cannot fail to have some unknown connotation. While the study is methodologically neat, the rationale underlying the measuring instrument is not convincing to this reviewer.

Prejudice and personality.—The University of California investigations of authoritarianism and prejudice by Adorno, Frenkel-Brunswik, Levinson & Sanford (2) have stimulated a number of related studies seeking to connect ethnocentrism with underlying cognitive and motivational dispositions.

Luchins (75) raises general objection to this approach, among other criticisms taking the original study to task for inadequate controls in the blind scoring of interview protocols. Converging evidence from a number of sources nevertheless continues to support and elaborate on the findings of the California investigators.

Gough, Harris, Martin & Edwards (44), in a questionnaire study of school children that employs variants of the California techniques, arrive at generally parallel findings. Going on to question the children's mothers with specially constructed scales, they found that a scale of authoritarian practices and attitudes differentiated between the parents of children high and low in prejudice at a level of borderline significance. Other parental scales were inconclusively related to the children's attitudes. Additional evidence for the association of prejudice and intolerance for ambiguity is provided by Block & Block (15), who found that subjects high in ethnocentrism were quicker than the relatively unprejudiced to establish a personal norm in repeated trials on the autokinetic phenomenon. In accounting for their results they prefer a conception of excessive ego control to that of intolerance for ambiguity.

Seeking a way of testing whether prejudiced attitudes themselves are rigid, Rokeach (95) investigates the related attribute of concreteness in a study of student's definitions of 10 religious and ideological concepts (isms). The least prejudiced quartile on an ethnocentrism scale tended to give more abstract and fewer concrete definitions. An additional finding, which he relates to the dogmatism often ascribed to the extremely unprejudiced, revealed more reification, a fallacy of abstraction, in the definitions of the low quartile. The latter finding meshes with the results of Dombrose & Levinson (28) from their use of a scale of ideological militancy versus pacifism with subjects receiving tolerant scores on ethnocentrism. For the tolerant group to which the scale was applicable, it correlated .74 with degree of ethnocentrism, leading them to conclude that

> those who strongly reject ethnocentrism tend toward militancy in their programs for the propagation and realization of democratic values, whereas those who moderately reject ethnocentrism tend toward pacifistic programs.

Thus a clearer picture is beginning to emerge of the low extreme in prejudice, which was left less clearly defined than the high extreme by the original California studies (2).

Because of their complete independence of the California studies and their reference to another national culture, the similar results reported by the English psychiatrist Dicks (27) from a study of German prisoners of war are particularly striking. On the basis of free psychiatric interviews, Dicks categorized 138 prisoners on a number of personality dimensions suggested by psychoanalytic theory. He compares the frequency of these traits among men independently classified by ideology into two groups: active or near Nazis, and unpoliticals or anti-Nazis. The Nazi group were significantly more likely to manifest Nazi or atheist religious views, lack of anti-father rebellion, transference of mother cathexis to Fuehrer or state, a taboo against

XUM

tenderness, anti-social handling of sadistic tendencies, and heightened homosexual trends, projectivity, and neurotic anxiety. Dicks attributes the dominance of the extreme Nazi position to the personality predisposition of the much more numerous middle groups to follow the lead of the militant Nazis.

A more directly psychoanalytic contribution is the volume by Ackerman & Jahoda (1) based on clinical summaries of 40 anti-Semitic patients in psychoanalysis. While the conclusions suffer from the absence of comparative material on unprejudiced neurotics (who must also find their way to the psychoanalyst), the study enriches with qualitative material the current psychoanalytic interpretation of prejudice. Mention should also be made of the case studies of prominent Nazis in Gilbert's recent book (41). Rejecting the view that the Nazi leaders were psychopathic personalities, he stresses the importance of "authoritarian regressiveness" at both the individual and the social level.

The reduction of prejudice and hostility.—A judicious and up-to-date summary of research on methods of changing attitudes is provided by Klineberg (70). Paralleling the emphases on individual and on sociocultural factors in the cause of prejudice are divergent but complementary approaches to its reduction.

The sociocultural emphasis is illustrated by the *ex post facto* experiment of Deutsch & Collins (26) in four interracial housing projects, two characterized by integrated and two by segregated residential patterns. Their results point to the effectiveness of the integrated pattern in providing equal-status neighborly contacts that are accompanied by more favorable white attitudes toward Negroes. They found no evidence that residence in segregated biracial projects has appreciable influence on intergroup attitudes or relations. Situational factors also furnish the avenue of attack in the action research project conducted by the Commission on Community Interrelations of the American Jewish Congress, on the training of people to answer anti-minority remarks. The most recent paper of a series [Seltiz *et al.*, (100)] finds that subjects obtained from the streets in Times Square and Newark to observe standard playlets preferred answers delivered in a persuasive rather than aggressive manner. Preferences among several lines of argument are explored with results that are not entirely consistent from group to group.

The therapeutic approach illustrated in a study by Haimowitz & Haimowitz (48) is one of the logical consequences of personality-centered views of prejudice. Before-and-after measures on a group of 24 counselling trainees in connection with six weeks of group therapy showed significant shifts toward greater friendliness to racial, religious, and low-status occupational groups, the shifts occurring primarily among those initially classified as mildly rather than strongly hostile or friendly. A somewhat surprising result, which if borne out on an adequate sample might be considered together with the previously reviewed findings of Rokeach (95) and of Dombrose & Levinson (28), was that the individuals who were judged to be the best counsellors fell most frequently in the intermediate, "mildly hostile" group.

The interplay of personal and social factors in changes of attitudes toward the Negro is the subject of a well-designed experiment by Mussen (84) conducted in an interracial camp for underprivileged children. Mussen's study is notable for relating underlying personality dispositions to changes in prejudice brought about by controlled social influences, in marked contrast to the more conventional research on static correlations between prejudice and personality. His basic data include results from the Horowitz Faces Test (on which he presents validation data (85)), the TAT scored for needs and press, and material on social adjustment collected from interviews and social workers' reports. While the camp experience did not result in over-all group changes in prejudice, coherent patterns of personality differences distinguished children who became more prejudiced from those who became less so. The detailed results support his three initial hypotheses:

> (*a*) Persons who have anti-Negro prejudice, but whose personality structures are such that they have little need for maintaining this prejudice, will relinquish their prejudice relatively easily; (*b*) those who relinquish their prejudices after intimate contact with Negroes give evidence of less concern with retaliation and punishment in general than those who maintain their prejudices; and (*c*) those who change their attitudes in accordance with the approved unprejudiced attitudes of a new group in which they find themselves are those who feel that the new situation is a rewarding one and hence make good social adjustments to it.

Intimate contact with a previously disliked group, it would seem, does not in itself guarantee a decrease in prejudice.

Opinion and Attitude Methodology

Methodological investigations instituted during and since the war are now paying returns at a rate that is rapidly transforming the repertory of techniques available for opinion and attitude research and are beginning to have repercussions on attitude theory. Only recently, the study of public opinion was pursued almost entirely by rule-of-thumb procedures, while the techniques of academic attitude studies were little more complex. Today, even though common practice lags considerably behind the methodological frontier, competence in this field requires qualifications in such diverse specialties as mathematical scaling theory, factor analysis, and the principles of dynamic psychology underlying projective techniques. Nor is methodological nicety being pursued in a vacuum for its own sake; some of the important technical advances have taken place in response to the pressing requirements of substantive research.

The classified results of the first decade of systematic public opinion polling have been published in a volume under the editorship of Cantril (19). Much of the experience represented in the thousands of questions included in this collection is funded in Payne's brief but comprehensive discussion of question wording (89). While Payne's book is studded with examples from polling experience, it does not belong among the deliberately limited but more systematic methodological contributions reviewed below. Kendall & Lazarsfeld (67) present a valuable codification of current good

practice in the analysis of survey results, treating the logic of causal inference as it applies to different types of survey data. They also discuss how survey data may be brought to bear on complex phenomena where indices play an important part and intrinsic checks are necessary if interpretations are to be dependable.

The problem of validity, sometimes put in terms of the relation between verbal attitudes and behavior, remains of perennial concern. Yet there is little support for a frontal attack on the gap between opinion and action; too many intervening variables are likely to be ignored for investigations of validity in this sense to have much theoretical significance. Studies of situational and personal factors deflecting verbal and other overt behavior are gradually accumulating the information needed if dependable prediction is to become possible.

Data bearing on the opinion and action problem are offered by Pace (88) from a study among a large sample of college alumni of the correlations between scales and items in several opinion areas, on the one hand, and related activity scales on the other. While low positive correlations were obtained for the most part, the strictures just made apply to the study. Pace's results have practical significance, but do not forward understanding of the intervening processes on which rational prediction must hinge.

Research during the past year has borne most actively on three problems: (*a*) the study of factors in the interviewing situation leading to bias in survey results, (*b*) the development and application of improved mathematical models for the measurement of attitude, and (*c*) the development of indirect and projective techniques to give greater depth to attitude study and free it from limits set by the insight and co-operativeness of the respondent.

Interviewing problems.—Most of the significant recent work on survey interviewing has appeared under the sponsorship of the National Opinion Research Center project on the measurement, isolation, and control of interviewer effect. Whatever the eventual verdict as to the characteristics of the ideal interviewer, practical limits are set by the fact that most interviewing in the public opinion field is done as part-time work for low remuneration. Sheatsley (102) has therefore analyzed the records and experience of the National Opinion Research Center and of other agencies when available, for insight into the nature of the pool of interviewers on whose competence or fallibility so much contemporary research depends. The implications of his findings for the problem of interviewer bias are still for the most part imponderable:

> We have a condition in which the great bulk of market and opinion research interviewing today is conducted by women talking to men, by college graduates talking to the uneducated, by upper-middle-class individuals talking to those of low socio-economic status, by younger people talking to the increasingly larger older-age groups, by white persons talking to Negroes, and by city dwellers talking to rural folk.

Examination of the characteristics of interviewing staffs in relation to hiring practices leads him to conclude that "the type of interviewers a company

hires is dictated almost entirely by the size and frequency of its assignments and by the type of work it does." The preferences of the persons doing the hiring appear to have had very little effect; demographic characteristics of a staff of interviewers have also remained remarkably stable from year to year in spite of very high turnover.

Three studies employ experimental procedures to attack the rather special problem of bias introduced in the process of recording a respondent's answers. All follow the design of presenting interviewer-subjects with standard stimulus material to record in an artificial situation. The expectations of the interviewers as a potential source of recording bias are investigated by Smith & Hyman (104), in the most elaborate study of the group. The subjects, including regular interviewers, heard and recorded on a normal interview schedule transcriptions of mock interviews with an isolationist and an interventionist respondent. The analysis focusses on the recording of critical responses designed, and independently categorized by judges, as equivalent in the two interviews. The biasing influence of expectations is seen in a "halo effect" deflecting the recorded critical responses toward the predominant ideology of the interview as a whole. Striking influences were demonstrated, giving rise in the recorded categories for one response to differences of as much as 55 per cent between the isolationist and interventionist contexts. The expectation effect was considerably greater than the effect of the subject's own ideology. Proneness to bias of this sort is shown to go with inaccurate performance in the field, but was unrelated to interviewing experience or clerical ability.

Guest & Nuckols (46) set out to study the effect on recording error of personality variables, clerical ability, intelligence, and the direction of the subject's own opinions. Aside from some evidence that recording error was more frequent on free response than on poll-type questions and was negatively correlated with intelligence test score, the results were mainly negative. Fisher (34) worked with highly artificial materials designed to maximize the likelihood of recording bias. The free responses to be recorded were built up of systematically differing proportions of pro and con statements. In this situation, subjects tended to record significantly more of the statements that agreed with their own opinions, the effect being greater on equivocal responses than on univocal ones.

The problem of respondents who are not at home or refuse to be interviewed is a serious one in probability sampling, as Crossley & Fink (25) have shown anew. Benson, Booman & Clark (14) demonstrated that follow-up interviewers with special training and qualifications (female beauty, for one) can complete virtually all the interviews of a fixed-address sample. They describe the cost in time and call-backs.

Problems of unidimensional measurement.—Theory and practice in regard to the mathematical problems of measurement in attitude research are in a state of flux, with contending theoretical models still in rapid development and research practice only beginning to apply some of the newer techniques. What is clear is that the traditional methods of attitude scaling are in the

process of being supplanted by ones with sounder mathematical rationale and, therefore, with greater potential relevance to psychological theory. Two of the principal contending approaches, Guttman's scale analysis and Lazarsfeld's theory of latent structure, are presented at length in the recently published volume by Stouffer *et al.* (106) that reports on methodological advances by the Research Branch of the Information and Education Division in the War Department. The more firmly established methods of factor analysis constitute a third approach.

Guttman's scale analysis (106) seeks order in the manifest pattern of responses to a series of items. The unidimensionality of an area of qualitative content, according to Guttman's hypothesis, is established if the items can be so ordered that "persons who answer a given question favorably all have higher ranks on the scale than persons who answer the same question unfavorably." The latent structure theory, mathematically more general but less fully developed toward specific application than Guttman's, also provides a way of ranking individuals unidimensionally, without requiring the rigidly cumulative pattern of response essential to the true Guttman scale. The basic postulate of this method (106), a generalization of the assumptions of factor analysis to apply to qualitative attributes, is that

> there exists a set of latent classes, such that the manifest relationship between any two or more items on a questionnaire can be accounted for by the existence of these latent classes and by these alone.

Various additional assumptions generate special cases capable of application to particular bodies of data.

Ford (36) gives a full description of a technique he has developed for Guttman scaling with the aid of IBM equipment. Several recent articles illustrate the utility of the Guttman technique in survey research, among them one by Foa (35), who also demonstrates the use of intensity analysis to determine an invariant zero-point on the content continuum. Evidence for the psychological validity of this method of locating a zero-point, which has been offered as a badly needed solution to the problem of bias in question wording, is adduced in his finding that the zero-point in terms of minimal intensity is also the point of inflection of the regression line in curvilinear correlations with outside variables.

Lazarsfeld's latent structure model would seem to fit some of the requirements of a psychological position that regards attitudes as inferential constructs rather than properties of manifest responses. An example of the method in application may be found in the previously reviewed article by Stouffer & Toby (107).

Eysenck and Cattell have been the principal exponents of factor analysis in the study of attitudes. The former has recently entered into controversy with Guttman on the relative merits of scale and factor analysis in attitude research (31), the issues being too complex and technical for review here. Eysenck puts factor analysis to use in the testing of hypotheses, as in a recent study (30) in which he investigated the relationship of measures of social insight and neurotic tendency to his basic orthogonal dimensions of

social attitudes, radicalism and tendermindedness. In support of the radical position he finds that social insight was positively related and neurotic tendency unrelated to radicalism. Cattell (20), on the other hand, is applying factor analysis in this field as in others in the service of basic taxonomy. From analysis of intercorrelations among indirect measures of a variety of attitudes and sentiments, he arrives at a somewhat McDougallian set of fundamental directive factors or "ergs."

Indirect and projective approaches.—The current somewhat uncritical enthusiasm for projective methods is reflected in the continuing addition of indirect techniques to the repertory of attitude research. While their standardization typically presents difficulties, they have at their best the twin advantages of disguise and depth. As Weschler & Bernberg (112) remark in a recent review of the literature, however, their future standing will depend "upon the type of research which will stress methodology rather than originality and scientific technique rather than ingenuity of design."

The error-choice technique, which in the guise of an information test lures subjects into choosing the one of two false or nonfactual alternatives more in keeping with their attitude, has been adapted by Weschler (111) to the investigation of attitudes toward labor and management. He presents validation data from appropriate criterion groups, but notes that the nature of the technique requires topical content that precludes standardization as an established test. In Sanford's study (97), already cited, of the follower's orientation toward authority, he makes effective use of an adaptation of Rosenzweig's picture-association method (96), also presenting internal evidence for validity. Under the topic of social perception, Haire's ingenious application of the Asch technique (49) has been previously noted. Haire has used this method, in which respondents project intuitive personality sketches on the basis of listed traits, in studying the dynamic coherence of stereotypes in several applied settings.

The most ambitious program of indirect attitude measurement is surely that of Cattell and his associates (23), for whom the attraction of the indirect approaches lies in their objective independence of introspective self-rating rather than in their association with depth psychology. In an interesting catalogue of potential techniques, Cattell classifies 25 possible measures, most of them previously untried, under the following rubrics: (*a*) interactive criterion methods (including allocation of time and money), (*b*) solipsistic criterion methods (including the classical opinionaire and paired comparisons), (*c*) attention-memory methods, (*d*) methods appraising cognitive and dynamic structure due to interests, (*e*) autism methods, (*f*) psychological activity level methods, and (*g*) physiological activity level methods. Cattell's group have investigated the validity of a large number of new techniques from this list, correlating scores on a brief 10-item form of each untried approach with scores on a pool of the methods that they regard as more dependable. Coefficients between .10 and .20, which they regard as giving promise of validity in view of the brevity of the tests, were obtained for eight new methods, including distraction, false belief, speed of decision,

information, projection, preference, immediate memory, and galvanic skin response. Because of the degree of specificity that they predict will remain even if the tests are lengthened, they recommend their use in a battery of from four to six different methods.

The methodological or clinical intricacies of approaches like Cattell's and some of the projective techniques may have, as it appears to this reviewer, an almost insidious appeal to gadget- and gimmick-minded American psychologists, if they distract attention from the human object of investigation. The interview, too, can be interpreted projectively, as it seems easy to forget in spite of the psychoanalytic origins of the projective approach. Riesman & Glazer (94) offer an example of how much can be extracted—granted a certain willing suspension of disbelief—from rather meager interview material. While most social psychologists would hesitate to place much confidence in so highly intuitive an approach as theirs, it should nonetheless contribute toward a productive balance in the methods of investigating attitudes.

LITERATURE CITED

1. Ackerman, N. W., and Jahoda, M., *Anti-Semitism and Emotional Disorder* (Harper & Brothers, New York, N. Y., 135 pp., 1950)
2. Adorno, T. W., Frenkel-Brunswik, E., Levinson, D. J., and Sanford, R. N., *The Authoritarian Personality* (Harper & Brothers, New York, N. Y., 990 pp., 1950)
3. Allport, G. W., *J. Social Issues*, Suppl. 4, 1–26 (1950)
4. Allport, G. W., and Postman, L., *The Psychology of Rumor* (Henry Holt & Company, Inc., New York, N. Y., 247 pp., 1947)
5. Arensberg, C. M., in *Social Psychology at the Crossroads*, 324–52 (Rohrer, J. H., and Sherif, M., Eds., Harper & Brothers, New York, N. Y., 437 pp., 1951)
6. Asch, S. E., *J. Abnormal Social Psychol.*, **41**, 258–90 (1946)
7. Asch, S. E., in *Groups, Leadership and Men: Research in Human Relations*, 177–90 (Guetzkow, H., Ed., Carnegie Press, Pittsburgh, Pa., 293 pp., 1951)
8. Back, K. W., *J. Abnormal Social Psychol.*, **46**, 9–23 (1951)
9. Back, K., Festinger, L., Hymovitch, B., Kelley, H., Schachter, S., and Thibaut, J., *Human Relations*, **3**, 307–12 (1950)
10. Barker, R. G., and Wright, H. F., in *Social Psychology at the Crossroads*, 196–212 (Rohrer, J. H., and Sherif, M., Eds., Harper & Brothers, New York, N. Y., 437 pp., 1951)
11. Bartlett, F. C., *Remembering. A Study in Experimental and Social Psychology* (Cambridge University Press, Cambridge, England, 317 pp., 1950)
12. Bavelas, A., *Applied Anthropol.*, **7**, 16–30 (1948)
13. Bell, G. B., and French, R. L., *J. Abnormal Social Psychol.*, **45**, 764–67 (1950)
14. Benson, S., Booman, W. P., and Clark, K. E., *J. Applied Psychol.*, **35**, 116–19 (1951)
15. Block, J(ack), and Block, J(eanne), *J. Personality*, **19**, 303–11 (1951)
16. Bruner, J. S., in *Perception: An Approach to Personality*, 121–47 (Blake, R. R., and Ramsey, G. V., Eds., The Ronald Press Co., New York, N. Y., 442 pp. 1951)
17. Bruner, J. S., and Postman, L., *J. Social Psychol.*, **27**, 203–8 (1948)
18. Cantril, H., Ed., *Tensions that Cause Wars* (Univ. of Illinois Press, Urbana, Ill., 303 pp., 1950)

19. Cantril, H., Ed., *Public Opinion, 1935–1946* (Princeton Univ. Press, Princeton, N. J., 1191 pp., 1951)
20. Cattell, R. B., *J. Abnormal Social Psychol.*, **45**, 598–618 (1950)
21. Cattell, R. B., *J. Social Psychol.*, **32**, 215–53 (1950)
22. Cattell, R. B., in *Groups, Leadership and Men: Research in Human Relations*, 16–27 (Guetzkow, H., Ed., Carnegie Press, Pittsburgh, Pa., 293 pp., 1951)
23. Cattell, R. B., Heist, A. B., Heist, P. A., and Stewart, R. G., *Educ. Psychol. Measurement*, **10**, 224–48 (1950)
24. Centers, R., *Intern. J. Opinion Attitude Research*, **4**, 315–35 (1950)
25. Crossley, H. M., and Fink, R., *Intern. J. Opinion Attitude Research*, **5**, 1–19 (1951)
26. Deutsch, M., and Collins, M. E., *Interracial Housing: A Psychological Evaluation of a Social Experiment* (Univ. of Minnesota Press, Minneapolis, Minn., 173 pp., 1951)
27. Dicks, H. V., *Human Relations*, **3**, 111–54 (1950)
28. Dombrose, L. A., and Levinson, D. J., *J. Social Psychol.*, **32**, 101–13 (1950)
29. Erikson, E. H., *Childhood and Society* (W. W. Norton & Co., Inc., New York, N. Y., 397 pp., 1950)
30. Eysenck, H. J., *Brit. J. Psychol., Gen. Sect.*, **42**, 114–22 (1951)
31. Eysenck, H. J., and Guttman, L., *Intern. J. Opinion Attitude Research*, **5**, 95–120 (1951)
32. Festinger, L., *Psychol. Rev.*, **57**, 271–82 (1950)
33. Festinger, L., and Thibaut, J., *J. Abnormal Social Psychol.*, **46**, 92–99 (1951)
34. Fisher, N., *Intern. J. Opinion Attitude Research*, **4**, 391–411(1950)
35. Foa, U. G., *Intern. J. Opinion Attitude Research*, **4**, 192–208 (1950)
36. Ford, R. N., *Public Opinion Quart.*, **14**, 507–32 (1950)
37. Ford Foundation, *Report of the Trustees of the Ford Foundation*, 23 pp. (Sept. 27, 1950)
38. Fromm, E., *Man for Himself* (Rinehart & Co., New York, N. Y., 254 pp., 1947)
39. Gage, N. L., and Succi, G., *J. Educ. Psychol.*, **42**, 144–52 (1951)
40. Garceau, O., *Am. Political Sci. Rev.*, **45**, 69–85 (1951)
41. Gilbert, G. M., *The Psychology of Dictatorship* (The Ronald Press Co., New York, N. Y., 327 pp., 1950)
42. Gilbert, G. M., *J. Abnormal Social Psychol.*, **46**, 245–54 (1951)
43. Gorer, G., and Rickman, J., *The People of Great Russia: A Psychological Study* (The Chanticleer Press, New York, N. Y., 236 pp., 1950)
44. Gough, H. G., Harris, D. B., Martin, W. E., and Edwards, M., *Child Development*, **21**, 83–91, 169–81 (1950)
45. Grace, H. A., *J. Educ. Psychol.*, **42**, 206–14 (1951)
46. Guest, L., and Nuckols, R., *Intern. J. Opinion Attitude Research*, **4**, 336–52 (1950)
47. Guetzkow, H., Ed., *Groups, Leadership and Men: Research in Human Relations* (Carnegie Press, Pittsburgh, Pa., 293 pp., 1951)
48. Haimowitz, M. L., and Haimowitz, N. R., *J. Social Psychol.*, **31**, 231–41 (1950)
49. Haire, M., and Grunes, W. F., *Human Relations*, **3**, 403–12 (1950)
50. Hayes, S. P., Jr., *Psychol. Bull.*, **47**, 289–330 (1950)
51. Hemphill, J. K., *Situational Factors in Leadership* (Bur. Educ. Res. Monographs, No. 32, Ohio State Univ., Columbus, Ohio, 136 pp., 1949)
52. Hemphill, J. K., *J. Social Psychol.*, **32**, 11–22 (1950)
53. Hemphill, J. K., and Westie, C. M., *J. Psychol.*, **29**, 325–42 (1950)
54. Higham, T. M., *Brit. J. Psychol., Gen. Sect.*, **42**, 42–55 (1951)
55. Hites, R. W., and Campbell, D. T., *J. Social Psychol.*, **32**, 95–100 (1950)

56. Homans, G. C., *The Human Group* (Harcourt, Brace & Co., New York, N. Y., 484 pp., 1950)
57. Howes, D. H., and Solomon, R. L., *Psychol. Rev.*, **57**, 229–34 (1950)
58. Hyman, H., *Arch. Psychol.*, No. 269, 94 pp. (1942)
59. James, H. E. O., and Tenen, C., *Brit. J. Psychol., Gen. Sect.*, **41**, 145–72 (1950)
60. Kardiner, A., and Ovesey, L., *The Mark of Oppression: A Psycho-social Study of the American Negro* (W. W. Norton & Co., Inc., New York, N. Y., 396 pp., 1951)
61. Katona, G., *Psychological Analysis of Economic Behavior* (McGraw-Hill Book Co., New York, N. Y., 347 pp., 1951)
62. Katz, D., *Ann. Rev. Psychol.*, **2**, 137–72 (1951)
63. Katz, D., and Braly, K. W., *J. Abnormal Social Psychol.*, **28**, 280–90 (1933)
64. Katz, D., Kahn, R. L., Jacobson, E., Morse, N. C., and Campbell, A., in *Groups, Leadership and Men: Research in Human Relations*, 68–105 (Guetzkow, H., Ed., Carnegie Press, Pittsburgh, Pa., 293 pp., 1951)
65. Kelley, H. H., *J. Personality*, **18**, 431–39 (1950)
66. Kelley, H. H., *Human Relations*, **4**, 39–56 (1951)
67. Kendall, P. L., and Lazarsfeld, P. F., in *Continuities in Social Research: Studies in the Scope and Method of "The American Soldier,"* 133–96 (Merton, R. K., and Lazarsfeld, P. F., Eds., The Free Press, Glencoe, Ill., 255 pp., 1950)
68. Kitt, A. S., and Gleicher, D. B., *Public Opinion Quart.*, **14**, 393–412 (1950)
69. Klein, G. S., Schlesinger, H. J., and Meister, D. E., *Psychol. Rev.*, **58**, 96–112 (1951)
70. Klineberg, O., "Tensions Affecting International Understanding: A Survey of Research," *Social Sci. Research Council Bull.*, **62**, 1–227 (1950)
71. Lazarus, R. S., and McCleary, R. A., *Psychol. Rev.*, **58**, 113–22 (1951)
72. Leavitt, H. J., *J. Abnormal Social Psychol.*, **46**, 38–50 (1951)
73. Lewin, K., *Field Theory in Social Science: Selected Theoretical Papers* (Cartwright, D., Ed., Harper & Brothers, New York, N. Y., 346 pp., 1951)
74. Lewin, K., Lippitt, R., and White, R. K., *J. Social Psychol.*, **10**, 271–99 (1939)
75. Luchins, A. S., *J. Social Psychol.*, **32**, 79–94 (1950)
76. MacLeod, R. B., in *Social Psychology at the Crossroads*, 215–41 (Rohrer, J. H., and Sherif, M., Eds., Harper & Brothers, New York, N. Y., 437 pp., 1951)
77. Marquis, D. G., Guetzkow, H., and Heyns, R. W., in *Groups, Leadership and Men: Research in Human Relations*, 55–67 (Guetzkow, H., Ed., Carnegie Press, Pittsburgh, Pa., 293 pp., 1951)
78. McClelland, D. C., *Personality* (William Sloane Associates, New York, N. Y., 654 pp., 1951)
79. Mead, M., in *Groups, Leadership and Men: Research in Human Relations*, 106–17 (Guetzkow, H., Ed., Carnegie Press, Pittsburgh, Pa., 293 pp., 1951)
80. Merton, R. K., and Kitt, A. S., in *Continuities in Social Research: Studies in the Scope and Method of "The American Soldier,"* 40–105 (Merton, R. K., and Lazarsfeld, P. F., Eds., The Free Press, Glencoe, Ill., 255 pp., 1950)
81. Morlan, G. K., *J. Genetic Psychol.*, **77**, 257–65 (1950)
82. Morris, R. T., and Seeman, M., *Am. J. Sociology*, **56**, 149–55 (1950)
83. Murdock, G. P., Ford, C. S., Hudson, A. E., Kennedy, R., Simmons, L. W., and Whiting, J. W. M., "Outline of Cultural Materials" (3rd rev. ed.), *Behavior Science Outlines*, **1** (Human Relations Area Files, Inc., New Haven, Conn., 162 pp., 1950)
84. Mussen, P. H., *J. Abnormal Social Psychol.*, **45**, 423–41 (1950)
85. Mussen, P. H., *J. Abnormal Social Psychol.*, **45**, 504–6 (1950)

86. Newcomb, T. M., in *Social Psychology at the Crossroads*, 31–49 (Rohrer, J. H., and Sherif, M., Eds., Harper & Brothers, New York, N. Y., 437 pp., 1951)
87. Ort, R. S., *J. Abnormal Social Psychol.*, **45**, 691–99 (1950)
88. Pace, C. R., *Educ. Psychol. Measurement*, **10**, 411–19 (1950)
89. Payne, S. L., *The Art of Asking Questions* (Princeton Univ. Press, Princeton, N. J., 249 pp., 1951)
90. Phillips, E. L., *J. Genetic Psychol.*, **77**, 61–72 (1950)
91. Polansky, N., Lippitt, R., and Redl, F., *Human Relations*, **3**, 319–48 (1950)
92. Postman, L., in *Social Psychology at the Crossroads*, 242–72 (Rohrer, J. H., and Sherif, M., Eds., Harper & Brothers, New York, N. Y., 437 pp., 1951)
93. Riesman, D., *The Lonely Crowd: A Study of the Changing American Character* (Yale Univ. Press, New Haven, Conn., 386 pp., 1950)
94. Riesman, D., and Glazer, N., *Intern. J. Opinion Attitude Research*, **4**, 515–40 (1950–51); **5**, 53–78 (1951)
95. Rokeach, M., *J. Abnormal Social Psychol.*, **46**, 83–91 (1951)
96. Rosenzweig, S., *J. Personality*, **14**, 3–23 (1945)
97. Sanford, F. H., *Authoritarianism and Leadership: A Study of the Follower's Orientation to Authority* (Inst. for Research in Human Relations, Philadelphia, Pa., 189 pp., 1950)
98. Schachter, S., *J. Abnormal Social Psychol.*, **46**, 190–207 (1951)
99. Seeman, M., in *Leadership in American Education*, 40–50 (Grace, A. G., Ed., Univ. of Chicago Press, Chicago, Ill., 1950)
100. Seltiz, C., Citron, A. F., Harding, J., Rosahn, O., and Wormser, M. H., *Intern. J. Opinion Attitude Research*, **4**, 353–90 (1950)
101. Shartle, C., in *Groups, Leadership and Men: Research in Human Relations*, 199–33 (Guetzkow, H., Ed., Carnegie Press, Pittsburgh, Pa., 293 pp., 1951)
102. Sheatsley, P. B., *Intern. J. Opinion Attitude Research*, **4**, 473–98 (1950–51); **5**, 79–94 (1951)
103. Sherif, M., in *Social Psychology at the Crossroads*, 388–424 (Rohrer, J. H., and Sherif, M., Eds., Harper & Brothers., New York, N. Y., 437 pp., 1951)
104. Smith, H. L., and Hyman, H., *Public Opinion Quart.*, **14**, 491–506 (1950)
105. Stouffer, S. A., Suchman, E. A., De Vinney, L. C., Star, S. A., and Williams, R. M., Jr., *The American Soldier*, **I**; Stouffer, S. A., Lumsdaine, A. A., Lumsdaine, M. H., Williams, R. M., Jr., Smith, M. B., Janis, I. L., Star, S. A., and Cottrell, L. S., Jr., **II** (Princeton Univ. Press, Princeton, N. J., 599 pp. and 675 pp., 1949)
106. Stouffer, S. A., Guttman, L., Suchman, E. A., Lazarsfeld, P. F., Star, S. A., and Clausen, J. A., *Measurement and Prediction* (Princeton Univ. Press, Princeton, N. J., 756 pp., 1950)
107. Stouffer, S. A., and Toby, J., *Am. J. Sociology*, **56**, 395–406 (1951)
108. Thibaut, J., *Human Relations*, **3**, 251–78 (1950)
109. Thistlethwaite, D., *J. Abnormal Social Psychol.*, **45**, 442–58 (1950)
110. Volkman, J., in *Social Psychology at the Crossroads*, 273–94 (Rohrer, J. H., and Sherif, M., Eds., Harper & Brothers, New York, N. Y., 437 pp., 1951)
111. Weschler, I. R., *J. Social Psychol.*, **32**, 51–69 (1950)
112. Weschler, I. R., and Bernberg, R. E., *Intern. J. Opinion Attitude Research*, **4**, 209–28 (1950)
113. Wolfenstein, M., and Leites, N., *The Movies: A Psychological Study* (The Free Press, Glencoe, Ill., 316 pp., 1950)

INDUSTRIAL PSYCHOLOGY[1]

By Clarence W. Brown and Edwin E. Ghiselli
Department of Psychology, University of California, Berkeley, California

During the past year, as in previous years, more of the scientific articles in industrial psychology have been concerned with the use of tests in personnel selection than with any other area. To the reviewers, this appears to be somewhat out of proportion to the needs. Although it is undesirable to gear the production of research in any area, whether pure or applied, to the current needs, it should do no harm to point up certain areas which seem to have importance beyond what is revealed in the number and quality of the relevant studies being conducted.

Present signs indicate that far more attention should be given to the problems of classification or placement. The great strides made during World War II in the development of selection tests have continued to stimulate studies in the construction of additional selection tests. However, the problems of classification, which were even of greater importance during the war, but which were less effectively solved, seem to have been lost sight of by workers in industrial psychology.

A second area where little scientific research has been accomplished during the past year is that of training. A large number of practical suggestions on training procedures can be found in the trade journals, but these are usually directed to the solution of immediate and individual problems. On the whole, these studies are not set up to discover generalizable principles and are not conducted under the rigorous control procedures demanded by experimental psychology. Although the area of learning in general psychology is one of the richest in fruitful application, there are large numbers of industrial learning problems still awaiting precise answers. It should be possible to attack many of these training problems in the psychological laboratory.

The reviewers would like to call attention to the need of more accurate reporting of the statistical and experimental characteristics of research studies. The primary purpose served by publication is to widen the utilization of the generalizations evolved. The accuracy of application of any generalization directly depends upon the reader's understanding of the statistical and experimental procedures utilized in the research.

It is not always clearly recognized that the success of an industrial psychologist is dependent upon his knowledge of industry quite as much as upon his understanding of principles of psychology. Having been brought up for several years on a concentrated diet of scientific method, he usually is compelled to work with people who do not think that experimentation is the sole

[1] The survey of the literature to which this review pertains was completed in June, 1951. The authors wish to express appreciation to Mr. James Yandell for invaluable assistance in the preparation of this review.

method of solving problems and who cannot present their problems in the language of his revered science. He then learns, sometimes for the first time, that he must adjust to the conditions in which he must work because he cannot adjust the working conditions to the textbook patterns which he has learned so thoroughly. Since many of the universities have accepted the responsibility of preparing psychologists for careers in industry, it behooves them to take counsel with those in industry who can speak authoritatively on the nature of the tasks which the embryonic industrial psychologist must face. Lawshe (74) arranged a symposium in which the contributions of industrial psychologists were appraised by three representatives from industry who had firsthand experience with their work. The industrial psychologist appears to be lacking in both understanding and sympathy for the group with which he is concerned. Furthermore, he is so convinced of his methods that he is impatient with those who are unfamiliar with them. As a consequence, he frequently fails to make them understandable to those with whom he must deal. Thus, while it is admitted that the psychologist can make contributions to industry, it is also clear that he is not sufficiently aware of the social environment in which he must work to effect the most favorable attitude in the people who will be immediately concerned with his contributions.

As in previous years since the war, the past year has seen many investigations conducted under the auspices of the armed forces. A large proportion of these studies can be considered to fall within the field of industrial psychology since they are concerned with such topics as job analysis, criteria, selection and classification, and human engineering. A large number of agencies in the Departments of the Army, Navy, and Air Forces are engaged in psychological research. Their findings are published in several special series and in a variety of separates. This situation, plus the fact that many reports are restricted, means that a large number of significant studies do not come to the attention of psychologists. However, it is sometimes possible through communication with these agencies to obtain lists of studies and abstracts. It is unfortunate that so few of these researches are reported in the usual professional psychological journals.

The periodical *Personnel Psychology* is performing a valuable service in facilitating the exchange of ideas and research findings through its regular articles by Carroll which summarize and discuss foreign publications in the field of industrial psychology. Many of the articles cited are in journals and bulletins which either are unfamiliar or are not easily available to psychologists in the United States.

Interview

Several psychologically significant articles on the interview procedure came to the attention of the reviewers. Daniels & Otis (27) give a comprehensive description of a method for analyzing various aspects of employment interviews. The technique has interesting implications for training interviewers as well as for evaluating their later effectiveness, not only in the

employment situation but in other interview situations where similar types of verbal exchanges are involved. Interviews were recorded electronically in the employment offices of eight companies, and a sample of 60 of them were analyzed. Some of the 26 categories used in analyzing the content of the interviews are: (*a*) time the interviewer spoke, (*b*) time the applicant spoke, (*c*) total time of pauses, (*d*) total number of exchanges, (*e*) number of questions asked regarding information already on the application blank, (*f*) number of answers by applicant, (*g*) number of "new information" answers, (*h*) volunteered information statements by applicant, (*i*) job information given by interviewer, and (*j*) number of questions asked by applicant. The authors supply a large number of coefficients of correlations between these categories. Some of the more interesting findings follow. The interviewer controls the length of the interview to a greater degree than does the applicant. The applicant has more control over the total number of exchanges than does the interviewer—the number of exchanges correlates with interviewer's time .42, with the applicant's time .65. The amount of talking by the interviewer varies with the number of questions he asks (.57), with the number of answers by the applicant (.57), with job and company information given the applicant (.38 and .37), with the amount of advice given the applicant (.60), with the number of questions the applicant asks (.48), and with the amount of unrelated material talked about (.30). The number of exchanges correlates .75 with the total time and −.28 with the mean time per exchange. Little time was wasted on "old information" questions and answers. Suggestions and advice to the applicant correlate .45 with the amount of information obtained in the interview. The effectiveness of a somewhat nondirective technique of obtaining information is shown by the fact that the number of volunteered information statements correlated .85 with the total number of interviewer's responses.

With the increased emphasis placed on the measurement of personality and leadership behavior in industrial situations, industrial psychologists should find Tupes' article (116) on unstructured appraisal interviews of some interest. As part of a larger assessment study of personality variables, initial appraisal interviews of 1 hr. and intensive appraisal interviews of 2 hr. were given students by expert clinical interviewers. Other credential and test material was available. The criterion was taken as the final pooled ratings of the two interviewers and a third person, who had administered tests to the students, using all test and interview information collected over a week's assessment program. The interview ratings and the criterion ratings were made on nine different personality traits. Each student was rated on these same nine traits using only credential data, and again using only credential and other objective data. The initial interview improved only slightly the rating based on credential materials alone. The longer intensive interview contributed little more to the assessment than did the shorter initial interview. The more comprehensive the information available to the raters the more valid the rating. Comprehensive psychological test data showed higher validity than interview data. It is to be noted that there was much contami-

nation of the data. Interviewers, interview data, and objective information are found common to the predictor ratings and the criterion ratings. The author acknowledges this contamination, but the reviewers would like to underscore this factor as one which seems characteristically to enter large-scale personality assessment studies and which, almost characteristically, is dismissed from evaluation in the analysis by the magic step of having been acknowledged. Until an effective contamination-removal formula or index is available, it is important that sound principles of experimental design be followed.

Selection and Placement Tests

General studies.—In reviewing the publications in the area of industrial selection, the reviewers noted several general characteristics. One was the failure of most authors to describe their studies in sufficient detail to enable the reader to make an accurate evaluation of the results. This confirms the findings of Jones (66), who says that most of the well over 2,100 studies she examined—published between 1906 and 1948—were deficient in reporting one or more of the general items needed for the reader to accurately assess the experimental results.

A second trend noted in the past year's reports was an increase in the construction and utilization of batteries of tests and the subsequent appraisal of these batteries by factor analysis and various correlational procedures. Super (110) calls attention to the development of standardized batteries devised to measure basic factors underlying a variety of jobs. Relatively pure tests are made possible through factor analysis, and these tests can then be combined into batteries and standardized on large samples representing a wide variety of occupations. Such batteries now in a developmental stage are the General Aptitude Test Battery of the United States Employment Service, the Differential Aptitude Tests of the Psychological Corporation, the Guilford–Zimmerman Aptitude Survey, and King's Factored Aptitude Tests.

Although the studies on the analyses of batteries of tests were often conducted on student populations, the information obtained should prove helpful to the industrial psychologist. An excellent factorial study of the Multiphasic, Strong, Kuder, and Bell inventories was made by Cottle (23), using 400 male veteran college students. Seven common factors were found in the four instruments. With one exception, there was no overlap between the personality inventories and the interest inventories. Two of the factors were common to the personality inventories and four common to the interest inventories. The results suggest the use of occupational pattern-analysis as a promising approach. A study of three personality inventories (Bernreuter, Guilford-Martin, and Guilford-Zimmerman) was made by Gilbert (45). She found high correlation between scales on different inventories which by psychological judgment would be considered similar.

Two factorial studies of interest to the industrial psychologist were made

on mechanical aptitude tests. Thurstone (114, 115) supplies data on 25 individual tests and 32 group tests selected to represent current ideas about mechanical aptitude and hypotheses concerning the nature of mechanical mental abilities. An interest schedule (Kuder), a test of mechanical experience representing tool knowledge, and a test of mechanical comprehension were added to constitute an informal criterion by which two groups could be formed, one group with high and the other with low interest and ability scores in these particular phases. Tables supply t-ratios for the differences between the two groups in all of the tests. The subjects were male technical high school students. Sixteen of the individual tests and 24 of the group tests gave t-ratios significant at the 1 per cent level or higher. Thurstone's second space factor is the most differentiating factor for mechanical aptitude, with Closure Factors 1 and 2, Space Factor 1, and Induction also contributing. Degan (30) refactored the Army Air Forces battery of mechanical tests using oblique rotation in order to compare the resulting factors with the factors obtained from the Navy Mechanical Aptitude Series. In the original Air Forces study, the configuration was rotated to an orthogonal simple structure rather than to an oblique structure as was done in the Navy study. Similarities and differences between the Army Air Forces' interpretation of factors and those obtained in the Navy's analysis are discussed.

Strong (109) announces the publication of means and standard deviations for men in general and women in general on the occupational scales of the revisions of his Vocational Interest Test. The data for men are found in the new Manual of Directions, and the data for women can be obtained from the American Documentation Institute. He also describes a method for computing the mean score of a group for a particular scale without scoring the individual blanks for that scale. There are discussions of various aspects of scoring the Strong Test, of the methods used in obtaining the normative data, and some data on the validity of the occupational scale in the form of percentages of overlapping of scores between the various occupational criterion groups and the populations of men in general and women in general.

The relationship between intelligence and occupational level was investigated by Simon & Levitt (104) using the Wechsler-Bellevue Test. Over 1,700 adults were examined. The usual relationship between occupational level and intelligence level was found, but the median intelligence scores for some of the lowest occupational groups were higher than the values obtained in previous similar studies.

A third general trend in selection testing is manifest in the development and use of personality tests in industrial selection and placement. The significance of interpersonal relationships in job performance is discussed by West (123). He cites the findings of five studies by the staff of the American Institute for Research in which the Critical Incidents Technique was used for discovering behaviors essential for successful job performance. The studies involved the following groups: (*a*) officers of the Air Force, (*b*) air route traffic controllers, (*c*) research personnel, (*d*) hourly wage workers, and

(*e*) dentists. The importance of personal factors in job success is indicated by the proportion of the reported critical behaviors which involved interpersonal relationships.

Particular interest has been shown in the group oral performance test (GOPT) sometimes referred to as a group interview. This test has been claimed to be superior to most others in the selection of leaders, but only recently have several attempts been made to determine its validity. Under the title of group interview tests, Fields (37) discusses its strengths, and Meyer (88) discusses its weaknesses. Their arguments are developed in reference to the individual interview. In the judgment of the reviewers, the group oral performance test does not replace the individual interview, and the test should be evaluated in terms of accepted principles of psychological measurement and not in relation to the interview. Douglas (31) critically reviews the use of the group oral performance test by the Civil Service Department of the State of New York. She points out five ways in which the examination abrogates sound principles of examining and develops two of these in detail: that the examination does not conform to the prerequisites of a psychological test instrument and that the New York examiners' concept of leadership is unsound psychologically and sociologically. Mandell (81) reports a study of leadership ability made on two groups of supervisors in Federal Government shipyards using the group oral performance test. The criterion was graphic ratings based on the collective opinion of the testees' colleagues and supervisors. Correlations of the GOPT with the criterion for the two groups was .36 and .21. Correlations of a Supervisory Judgment Test with the same criterion, using the same groups, was .47 and .43.

The most adequate validation of the GOPT that the reviewers have encountered is given by Vernon (118). His study was made on candidates for administrative and foreign service positions in England. It included a wide variety of measures such as individual interviews, group exercises (GOPT), a variety of tests, and extensive biographical data. Several follow-up studies were made. In general, the correlations between predictors and criteria were greater for group exercises (GOPT) than for tests and were greater for individual interviews than for group exercises. A more complete description of the study appears under the subtopic of Professional and Supervisory Tests.

The use of projective tests in industrial selection and placement is championed by Henry & Moore (58). Without presenting empirical justification for their arguments, these authors discuss the aspects of personality important for industry which are tested by the Rorschach, Thematic Apperception Test, and Henry-Moore Test of Thematic Production. Ammons, Butler & Herzig (2) describe a Projective Thematic Apperception Test constructed to measure vocational interests and attitudes and at the same time to give information concerning related psychological forces operating within the person lity. The test was tried out on 75 college subjects. In the reviewer's opinion, the authors' conclusions that the test has satisfactory reliability and validity are not justified from the meager data collected. Mitchell & Rothe (91) report the validation of an emotional key for a short industrial per-

sonality questionnaire. The scores differentiated between clinical patients and normal industrial employees.

Continued use of inventory and questionnaire procedures in the measurement of interest and personality has resulted in additional studies on the problem of faking answers. Although some investigations are conducted with student populations, they contribute to the solution of the general problem. Evidence on faking scores on the Kuder Preference Record is reported by Cross (24), using both male and female high school and college students. Significant changes were found on each of the nine scales, faking both up and down, for both high school and college students. Critical ratios (t) were very large in all but one or two instances.

Kuder (72) describes an extensive study of procedures for identifying the faker. He developed an honesty (H) scale and a verification (V) scale on the Preference Record—Personal. Using both scales improves the identification of the faker over using either one by itself. Cutting points were determined on one group and later applied to new groups. The scales identified faked blanks with more than 85 per cent accuracy except for 10 per cent of the scores which fell in a doubtful range of the scoring keys. In two articles, Rothe (99) and Carr & Rothe (16) discuss the use of an objectivity key in detecting and correcting for faking on a short industrial personnel questionnaire. The objectivity key is composed of six items. College students were shown to be able to fake their responses on the questionnaire and thus get a low objectivity score. The reviewers cannot agree with the authors' reasoning that a low objectivity score means that the respondent is faking his answers to the questionnaire. This may or may not be true, but there is insufficient evidence to justify this interpretation.

The only article on classification to come to the reviewers' attention was by Thorndike (113). He distinguishes the problem of classification or placement from the problem of selection. In its pure form, classification is concerned with the placing of job applicants when all of them must be used and when they must be assigned to each of several job categories. Two types of problems arise in the use of tests in classification: (*a*) problems involving the design, choice, and weighting of tests in a battery and (*b*) problems of establishing the optimum administrative procedure of using the test results in the assignment of the applicants. In reference to (*a*) Thorndike recommends the use of simple, factorially pure tests which may be expected to have a wide range of validities for different jobs. In assignment, problems arise concerning the kind of comparable unit that should be used for expressing predictions of success in different jobs. These units should take account of both predictor validity and job importance. Two procedures for making assignments are discussed, one in terms of daily quotas and another in terms of a stable predicted yield.

Professional and supervisory.—In devising an aptitude test for predicting success in veterinary medicine, Owens (94) studied preveterinary grades, the American Council on Education Test, the Moss Medical Aptitude Test, and three special tests (chemistry and zoology achievement, paragraph

comprehension, and verbal memory). The criterion was academic grades in the first part of veterinary medicine. Four student validation groups were used. The specially designed tests gave the highest correlations (.48 to .72). The reliability coefficient of the battery was .88. Ford (40) studied high school grades and several ability and interest tests as predictors of academic success in three nursing schools. The battery giving the most economical prediction ($R = .68$) was composed of reading comprehension, general science, and high school grade point average. Mandell (82) briefly summarizes the general success achieved in supervisory and executive selection by measuring instruments in the following areas: ability to appraise one's supervisees (Personal Analysis Test), interest in abstract and theoretical matters (Allport-Vernon), judgment in administrative problems (Administrative Judgment Test), leadership in group situations (Group Oral Performance Test), intelligence, attitude toward supervisees, and interest and aptitude in the field of work to be supervised. In another article, Mandell (83) reports validity coefficients ranging from .49 to .68 (with one exception of .28) for a test of adminstrative judgment given to government administrators. Two criteria were used: an average of four ratings on job performance by colleagues and superiors and the position grade or salary of the worker.

The most complete study of the selection of administrators is reported from England by Vernon (118). He describes validity studies for higher level Civil Service positions in the two classifications of administration and foreign service. Selection procedures included many varieties of objective tests, extensive biographical data, several individual interviews, group exercises or discussion sessions, over-all ratings by the Civil Service Examining Board of three judges, and final rating and selection by the Civil Service Commission Selection Board. Training and/or job proficiency criteria, based on ratings of superiors, were obtained after different periods of time, and groups were available for five validation studies. Complete statistical analyses were made of the data including the use of factor analysis and multiple correlation. Two of the outstanding results are: (*a*) 80 per cent of the variance of the rating of the Final Selection Board is accounted for in the earlier selection procedures and (*b*) corrected correlations between the Final Selection Board's rating and followup criteria in the five groups studied were .82, .50, .66, .62, and .56.

Fourteen factory supervisors were studied by Dulsky & Krout (33) to determine whether an employee's potentiality for promotion could be predicted. Tests used included the Wonderlic Personnel, Cardall Practical Judgment, Kuder Preference, Krout Personal Preference, and the Rorschach. Ratings were made by three executives of the supervisors' promotional potential. Two psychologists rated the supervisors' promotional potential in terms of the test performances. The ratings of the executives and psychologists correlated .60. The authors conclude that working with standardized objective data the psychologist can determine promotion potential. Lattin (73) studied graduates of the School of Hotel Administration at Cornell University in an attempt to differentiate successful from unsuccessful admin-

istrators. The subjects had been out of school over 14 years. Data were obtained on biographical characteristics, Egbert's A Study of Choices, and the Kuder Preference Record. Only a few promising leads were obtained from the 31 variables studied. A validity study of the Purdue mechanical aptitude test is reported by Rothe (100), using time-study trainees. The groups studied were very small. Ratings were made of the trainees' time-study ability. The r's varied from −.43 to +.50. Norms are presented for foremen, foremen applicants, and time-study applicants.

Clerical and office.—Holmes (59) administered a battery of clerical, ability, and personality tests to 269 supervisory, interpretative, and skilled office personnel. The criterion was appraisal of job performance by the workers' supervisors. The multiple R's ranged from .56 to .60, with no personality measures showing acceptable validity. In a second study (60), Holmes reports an investigation on secretaries, typists, and clerk typists. The Wonderlic and the typing and cancellation tests provided batteries for the three positions with multiple R's of .45, .42, and .45. Poruben (96) describes a validation study on five tests designed to predict success in actuarial clerk positions. Various tests were studied measuring mental alertness, numerical aptitude, and memory. The criteria were ratings by superiors on six traits and an over-all rating. A ratio-proportion test and the Westman Personnel Classification Test gave a multiple R of .46 with ratings of Ability to Learn. Hay (57) found that Minnesota Number Checking, Number Series Completion, and Name Finding Tests gave a multiple R of .38 for key punch operators. Using the same tests the multiple R for bookkeepers was .70.

Protective service employees.—Twenty-five New York City police patrolmen were evaluated by Kates (67) on the Strong Interest Test, a Hoppock-type job satisfaction blank and the Rorschach Test. Police interest ratings agreed with Strong's criterion group. The patrolmen scaled high in job satisfaction and appeared to be no more maladjusted (by Rorschach) than office clerks and biologists. Humm & Humm (63) report the appraisal of five groups of police officers in which the Humm-Wadsworth Temperament Scale was used. Statistics are presented to show the agreement between scale ratings and outcome of employment (promotion to staff, resigned, or discharged). The agreement was stated as averaging around a correlation of +.72. From Table 2 of the report, it can be shown that of those rated good and very good, 42 per cent remained on the job; of those rated fair or mediocre, 64 per cent remained; and of those rated poor and very poor, 41 per cent remained. The reviewers question whether these figures will justify a relationship as high as +.72.

An excellent study of selection of policemen was conducted by DuBois & Watson (32). They used 29 tests, inventory, and rated predictor variables which they checked against the following four criteria: final grade in the Police Academy, an achievement test on "Elements of Police Science," marksmanship, and service ratings after ten weeks on duty. Values of the multiple R's were as follows: .29 for service ratings, .33 for marksmanship, .59 for achievement test, and .60 and .58 for academic grades. When the regression

weights were used on a more recent class for cross validation with academic grades, the validity coefficient with academic grades was found to be .62.

Wolff & North (124), using 144 cases, did a preliminary study of selection tests for firemen. The tests included the Army Alpha, Kuder Preference, Bennett Mechanical Comprehension, and a specially constructed fire department achievement test. The firemen were ranked by their superiors on overall ability on the job. The Fire Department Achievement and the Bennett test gave the only significant differences between high-rated and low-rated groups. The average correlation between the Achievement Test and the criterion was .30.

Skilled and semiskilled.—McCormick & Winstanley (86) describe the construction of a 15 min. oral trade test for machinists. A validity of .53 with ratings of performance was demonstrated for first class machinists at a Naval shipyard. Ghiselli & Brown (44) present a tabulation of weighted mean validity coefficients of 10 different types of tests (intelligence, arithmetic, mechanical principles, etc.) for success in training of auto mechanics. They also report a study of arithmetic, spatial relations, and mechanical principles tests in which the criterion was ratings of performance by foreman and superintendent of 225 gasoline bus maintenance and repair men. The validity coefficients were as follows: arithmetic .19, spatial relations .21, and mechanical principles .30.

Poruben (97) reports the validation and standardization of the Adjutant General's Office general mechanical aptitudes test on 480 boys in a trade school. The criterion was grades in industrial and technical courses. For various groups the average multiple R was .39. The Medical Research Council of Great Britain (87) investigated two intelligence tests, a spatial relations test, and a planning and drawing test in the selection of students for the technical schools in the building trades. Rho between ranks on the tests and ranks in the course grades was .47. Prediction of trainability in auto mechanics and apparel design was studied by Martin (84). Ratings by instructors of personality and ability variables served as criteria against which a variety of ability and personality tests were checked. Multiple R's between different combinations of tests and the different criteria, significant at the 5 per cent level or better, varied from .40 to .79. Page (95) reports norms for the Purdue Industrial Mathematics Test and the Adaptability Test based on 42 metalsmiths and 51 general aviation mechanics. Reliability coefficients and intercorrelations of the tests are given.

Maslow (85) describes a written test developed for general helpers (semiskilled and unskilled workers); the sub-tests were arithmetic, counting, comparing names and numbers, and performance tests using tools and machines. Validation of the test is in progress. Jones & Smith (65) using employees in a meatpacking plant report that none of the scores on the Ortho-Rater is significantly related to job proficiency as measured by foremen's ratings. Lennon & Shapiro (77) using 40 food packing operators found a marked relation between individual production ratings and visual efficiency at 16 in., but no correlation was found between production and vision at 20 ft.

Kephart & Mason (69) studied the relation between the acuity differences between the two eyes and job performance on 5,600 workers in 92 industrial jobs. Acuity differences are not significantly related to job performance. Ely, Kephart & Tiffin (34) provide norms on the Ortho-Rater for 7,600 male and 2,500 female industrial employees.

Job Analysis and Evaluation

Job analysis.—Since Flanagan and the staff of the American Institute for Research (39) developed the Critical Incidents Technique, there have been no new contributions to the methodology of job analysis. During the past year, Christensen (19) proposed that the familiar device of sampling activity might be useful in job analysis. In obtaining quantitative data concerning the behavior of workers on a job, the usual technique is to time each activity element with a stop watch. In many situations, this is impossible because of the rapid succession of movements. Furthermore, it is impossible for an individual to carry on these observations over any long period of time. As a substitute, Christensen proposes what he terms "the sampling technique of activity analysis." In this approach, at regularly spaced and predetermined intervals of time, the observer notes any predefined elements of behavior that are occurring. These activity elements might be molar or molecular as deemed desirable. The major advantage of the method is that it permits a study of behavior in its natural setting. Its major disadvantage is that infrequently occurring behavior and reactions to emergency situations are likely to be missed. In two studies, it was found that samples taken every 60 sec. were about as effective as samples taken every 10 sec. Reliability coefficients for samples taken over a period of an hour at 5 sec. intervals were found to be of the order of .85.

The need for making a job analysis prior to the construction of a test is well recognized, but there have been few systematic attempts to integrate the procedures of job analysis and test development. A most significant beginning has been made by Wagner and the American Institute for Research (119) in a research project for the Department of the Air Force. This project is designed to test the feasibility of utilizing the Critical Incidents Technique as a basis for determining testable job elements, developing aptitude tests for these testable elements, and developing weights for tests in a classification battery. In preliminary work, it was found that a practical number of job elements can be defined and used as a framework for classifying critical behaviors, that these critical behaviors are determined with very high reliability, and that distinctive patterns of requirements emerge for different jobs on the basis of which differential weighting of tests might be accomplished.

Job evaluation.—The more or less recent interest of psychologists in job evaluation continued to manifest itself during the past year. Unfortunately, most of the concern was with methodological problems rather than with fundamental considerations and basic rationale. The recent article by Gomberg entitled "A Trade Unionist Looks at Job Evaluation" (46) can there-

fore be considered of prime importance. The author attempts to present for psychologists some of the rational bases of wage structure, the relationship between job evaluation and collective bargaining, and the philosophy of remuneration for work. It is pointed out that in many respects the bases of job evaluation are denied by the facts of individual differences and the principles of measurement. In a stimulating analysis from management's point of view, Knight (71) has pointed out various assumptions and fallacies in job evaluation. The purpose of job evaluation is stated as being the determination of the job's price or worth, and the basic assumption underlying present-day evaluation is that the value of a job should be proportional to the qualifications that the worker must possess and the sacrifices he must make. While this notion has long been accepted, it is pointed out that it is based upon a primitive theory of values which presumes that values once established are static. There is a real need for discussions such as those by Gomberg and Knight which attempt to provide a picture of the job evaluation problem in its larger perspective and thereby divert attention from studies dealing only with methodological details.

In recent years, several studies have indicated that the number of variables utilized in job evaluation can be reduced. Continuing studies in this area, Ash (4) performed a multiple factor analysis of eight factors used in the job evaluation of each of three classes of positions (clerical, subprofessional, and professional) involving nearly 800 positions. The two factors which emerged in all analyses were supervisory responsibility and general skill demands. However, other factors such as job complexity and nonsupervisory authority were also found, but were specific to the different classes of jobs. These findings demonstrate that the determiners of job worth are different for different types of jobs. In a very significant study, Davis & Tiffin (29) were able to show that weights derived for abbreviated job evaluation systems developed through factorial analyses hold up when applied in new situations. Cross validations of this kind, though most important, are seldom accomplished.

In a series of three papers, Gray (49, 50) and Gray & Jones (51) compared custom-made systems of job evaluation with ready-made systems. They suggest that systems of job evaluation can be validated by comparing the differences between the evaluations of key jobs (those that are known to carry proper wage rates) and the wages they actually carry. When the ratios of points assigned to key jobs by a job evaluation system correspond to the ratios of their salaries, then the system can be considered valid. In some instances these ratios can be made equivalent by adding constants to the points assigned. Utilizing this approach to validation, it is concluded that although custom-made systems of job evaluation require considerable effort to devise, this cost is justified in terms of the greater accuracy achieved. In a study of the factor-comparison job rating, Hay (56) shows that the factor values behave approximately in accordance with Weber's law. An increase in job difficulty at high levels calls for a larger salary increment than a similar increase at low levels although the proportional increase is the same.

He found in the construction of factor scales that "just noticeable differences" that were correctly discriminated by trained job raters 75 per cent of the time were 15 per cent apart in salary value. Accordingly, factor scales are to be set up with a 15 per cent increment in point values corresponding to uniform differences in job difficulty for the particular factor being rated.

Criteria

Two very significant contributions in the area of criteria have come from Brogden & Taylor. In their first article (11), they classify various kinds of biasing factors in criteria according to the following scheme: (*a*) criterion deficiency, omission of pertinent elements from the criterion, (*b*) criterion contamination, introducing extraneous elements into the criterion, (*c*) criterion scale unit bias, inequality of scale units in the criterion, and (*d*) criterion distortion, improper weighting in combining criterion elements. Specific problems of bias are discussed in relation to this classification and in relation to the following types of criteria: production records, ratings, and job sample measures. The article treats many of the problems encountered in criteria construction and gives valuable suggestions for minimizing the various kinds of criterion bias. In a second paper (12), Brogden & Taylor apply the concept of cost accounting to the construction of criteria. A worker's effectiveness is determined by his contribution to the objectives of overall efficiency of the organization, and this contribution can be best expressed in dollars and cents. Any criterion measure such as production, accidents, wastage, errors, etc., can be expressed in these units. The dollar unit serves as a numerical unit by which subcriteria can be placed on comparable scales, thus affording a basis for combining them. The variation in dollar values found for a given criterion can be used as a means for determining the weight to be assigned the criterion. Implications of the cost accounting concept for ratings and for problems in selection and classification are considered.

The influence of value judgments in the selection of criteria is discussed by Fiske (38). He calls attention to the fact that either the personnel psychologist or management must make value judgments in determining criteria for validating selection and training programs. He suggests that, where possible, value judgments be made concerning only the objectives of the whole organization, and then the purpose of any specific employee position can be described in terms of its contribution to this larger objective. Fiske suggests that such a procedure cannot be developed until there is an "adequate theory of human functioning and a comprehensive set of principles concerning interpersonal relationships." The reviewers believe that value judgments will continue to be required at all levels of an organization, not simply because our theoretical frameworks are inadequate, but because there will always be decisions to make involving motives, desires, likes, goals, etc. of employees, and these decisions will necessitate value judgments. In the future, such psychological behaviors as those mentioned will probably enter more rather than less into the construction of criteria which are to be used in validating selection and training programs.

An important contribution to criterion evaluation comes from Taylor & Wherry (112) who studied the relative amounts of bias found in graphic and forced-choice rating procedures. Bias was considered to be reflected in changes occurring in the mean values and the shapes of the distributions of ratings between two situations—one in which the ratings were made with the understanding that they would be used solely for research purposes (experimental), the other in which the ratings were to be used for administrative purposes (for keeps). The study involved hundreds of cases and utilized data obtained in the extensive research conducted in connection with the development of the Army Officer Efficiency Report. Many facts pertinent to the theory of criterion development were obtained. A few of these follow. The increase in mean values from the experimental to the for-keeps situation (leniency error) is much greater for graphic ratings than for forced-choice. The shape of the distribution curves for the forced-choice method is about the same in the two situations. For the graphic method, there is marked increase in skewness under the for-keeps situation, and the discrimination at the high end of the scale is greatly reduced.

The merit of using an immediately available training criterion to select a battery of tests to predict job success over a long time period was studied by Worbois (125). He examined the relationship between selection tests given in 1929 with performance ratings made by supervisors in 1948. The original criterion in 1929 was progress in learning the job. Using the Wherry-Doolittle method, the five tests giving the highest prediction with the original criterion were selected and the scores on these tests checked against the long range (1948) criterion. The Grimsley multiple cutting-score method was also used to select the three most predictive tests, and the scores on these tests checked against the 1948 criterion. Either method of developing the test battery on the 1929 data would have screened out three-fourths or more of the men who, in terms of arbitrary cutting scores on the 1948 ratings, turned out to be poor performers.

Bittner & Rundquist (10) have devised a combination of the methods of paired comparison and ranking for obtaining rating criteria. The procedure involves only a short time, can be used to evaluate large numbers of individuals, and does not require the assumptions that other procedures for combining these two methods have involved. In two studies, the reliabilities of the obtained ratings were .89 and .92, with one and three months elapsing between ratings, respectively.

A study of several methods of scoring a check-list type rating scale was made by Meyer (89). He used a sample of 200 completed forms made by two raters on 100 individuals in a study of "human relations" ability in supervision. The several ways of scoring were as follows: (*a*) median value of the scale values of the items endorsed, (*b*) sum of the deviations of the weights for the items endorsed from the mid-point on the scale, (*c*) sum of the endorsed favorable statements (weighted +1) and the endorsed unfavorable statements (weighted either −1 or −2 depending upon their power of discrimination), (*d*) sum of the endorsed favorable statements (weighted +1)

and the endorsed unfavorable statements (weighted −1), (*e*) number of favorable statements endorsed, and (*f*) number of unfavorable statements endorsed. The reliabilites of the methods were: (*a*) .87, (*b*) .82, (*d*) .82, (*f*) .72. Intercorrelations between methods ranged from .69 between methods (*e*) and (*f*) to .98 between methods (*b*) and (*c*). Theoretically, the greatest precision is achieved in method (*a*) in which the degree of favorableness or importance of each statement is taken into account. Logically, method (*b*) is superior. In the present study, however, practically the same results were obtained by method (*d*) which is far simpler than either methods (*a*) or (*b*). Between methods (*a*) and (*d*) the correlation was .93, and between methods (*b*) and (*d*) the correlation was .97. The further simplification found in methods (*e*) and (*f*) leads to a loss of precision which is not justified in terms of the greater ease of scoring.

Uhrbrock (117) used the method of equal appearing intervals to scale 540 statements describing performance. The statements were scaled separately by two groups of judges (technical assistants and foremen) in reference to success as a foreman, by two groups (students and teachers) in reference to success as a teacher, and by one group (teachers) in reference to success on any job. The intercorrelations of scale values from different groups of judges and odd-even reliabilities within groups were all .96 or above. The results indicate that as few as 10 expert judges can reliably scale such statements, that there is high agreement in scaling of judges of different occupational background, that such statements as those studied are judged to indicate similar degrees of success in widely differing types of work. Scale values and variances are provided for the 540 statements and for an additional 184 statements not included in the main study. Bayroff & Burke (6) describe a self-teaching guide on how to rate performance which was prepared for use by enlisted personnel in the Army. The language is kept simple and many illustrations are used. The guide is not for general distribution.

There has been considerable interest in merit rating procedures. Lott & Boddie (78) report a poll of 1,525 supervisors and 1,403 employees on the use of merit ratings. Although many critical opinions were expressed, a very large percentage felt that merit ratings served a practical purpose and should be continued. Bentley (8) presents reasons why the Federal uniform rating system failed and suggests steps to remedy the situation. He recommends that performance ratings not be made the basis for personnel actions such as promotion, pay increases, discharges, etc.; this use he feels was the reason for the failure of the Federal system. He argues that merit ratings are an essential part of a continuous evaluation of the effectiveness of an organization, with the two specific purposes of (*a*) improving supervision and (*b*) providing management with an inventory of personnel. Industry's viewpoint is presented by Schmidt (102). He gives 16 ways in which merit ratings can contribute, including selection, training, transfer, promotion, production, and morale. None of the authors above points out the need for ratings to be tailored to the specific purposes for which the results are to be used. Criticism should not be leveled at the rating instrument or at the procedure when

it is executed correctly, but rather at the attempt of personnel people to make one rating serve many different purposes.

Webster, Winn & Oliver (120), after reporting the defects of merit ratings made of employed engineers, proceed to perform a factor analysis of 100 of the ratings obtained on a five-point scale. The three following factors were extracted: (*a*) general appraisal of the inidvidual's promise for future success, (*b*) appraisal of the employee's present performance, and (*c*) evaluation of the impression the employee makes when meeting others. A model procedure for the construction of merit rating scales is found in an article by Ferguson (36). He describes the procedure used for constructing the scales for the Life Office Management Association for the rating of clerical employees. A careful selection was made of traits which were held important by first line supervisors and which also differentiated between workers. The traits had to be pertinent to job performance and had to be well known and have constant meaning. Many tables of empirical data are provided to support the procedures and justify the nature and content of the final scales.

Training

During the past year the area of training did not receive the attention that it warrants. Freyer & Edgerton (43) summarized the research in the armed services on instructor training, instruction technique, and training in communications. Freyer (42) sought to summarize and integrate ideas with respect to the improvement of training in complex jobs. He stresses the fact that the learning behavior of trainees needs to be studied and yet is neglected in job analysis. He also traces the development of methods used in programming for training, and attempts to demonstrate how descriptive items of learning behavior can be used as a basis for the evaluation of instructors or of training devices.

A significant contribution was made by Canter (15) with respect to methodology in the evaluation of human relations training programs. He sought to define and set up devices for measuring those dimensions of supervisory behavior which are likely to be amenable to change through the influence of training in human relations. The devices developed were a test of knowledge of general psychological principles, a questionnaire on supervisory practices, and a measure of the accuracy with which supervisors can estimate worker morale. These devices, together with certain standard procedures such as the File-Remmers questionnaire on how to supervise and the Watson-Glaser tests for critical thinking, were administered to 18 supervisors who received a systematic course in human relations and to 18 control subjects. While the numbers of cases were small, the principles of experimental design for the evaluation of such a training program are clearly illustrated and criterion problems are adequately pointed up. For the particular training program studied, it was found that significant improvements resulted in most of the criterion measures.

Motivation and Morale

The increasing interest in social and group factors in industry has brought about greater emphasis on role behavior as contrasted with individual factors in motivation and morale. Thus, Mace (79) in analyzing incentive systems sees social relations existing between labor and management as an essential consideration and joint participation as a necessity. He feels that, since the aim of incentive systems is to improve achievement of group determined goals, the common practice of linking incentives with individual goals is undesirable. Three essential conditions are outlined for linking productive effort with desired effects: (*a*) the goal of production must be intelligible to the worker, (*b*) the worker must be able to assess the degree to which the goal is attained, and (*c*) the worker must feel that he can contribute to the goal.

The number of grievances sometimes is considered to indicate the state of morale of workers arising out of the particular set of social relations existing between a management and a labor group. Hence, emphasis is placed upon the establishment of satisfactory grievance procedures as a means for reducing the extent of frustration existing among workers. However, Lawshe & Guion (75) point out that there is no generally accepted criterion of the worth of grievance procedures. By means of attitude questionnaire techniques, these investigators sought to discover the areas of agreement between management and labor on matters pertaining to grievances and the consensus of opinion in these areas. They found that a grievance must be defined broadly and should include imagined wrongs in addition to the mechanics of the company-union relationship. A grievance procedure should serve to encourage expression of grievances where they exist; therefore, it is possible that under a satisfactory grievance procedure the number of grievances might actually increase rather than decrease.

Over 600 workers in an aircraft factory were interviewed in their homes by Stockford & Kunze (108) in order to obtain data concerning their attitudes toward the company. It was found that those whose starting salary at the company was lower than the salary earned on the previous job had in general a less favorable attitude toward the company and were more likely to leave its employ. Furthermore, the larger the difference between previous salary and starting salary with the company the lower were the merit ratings assigned to them by supervisors.

Seidman, London & Karsh (103) found that the circumstances under which an individual joins a union become relatively unimportant where the union's method of operation, the status it achieves, or the pressure of events leads him later to become an active leader in the union. Such individuals identify with the union in a manner which indicates that it has become a way of life, serving the needs of its members in ways additional to the reasons that brought it into existence.

For many years the industrial psychologist has subscribed to the commonly accepted hypothesis that job satisfaction is closely related to the

worker's personal and emotional adjustment. In spite of a widespread acceptance of this hypothesis, little bona fide research has been conducted to explore its validity. Kates (68) investigated the problem by exploring the following four hypotheses: (*a*) job satisfaction in clerical work bears a significant positive relationship to scores on the office worker's scale of the Strong Vocational Interest Blank, (*b*) job satisfaction in clerical work is not related to Rorschach signs of maladjustment, (*c*) job dissatisfaction of clerical workers who have high scores on the Strong scale is not related to the number of Rorschach signs of maladjustment, and (*d*) certain Rorschach responses of clerical workers are significantly associated with high performance on the Strong scale. The subjects were 100 male government clerks. Job satisfaction was measured by the Hoppock Job Satisfaction Blank. The following findings are reported. Interest scores are correlated with job satisfaction to only a small extent ($r = +.21$). No apparent relationship exists between job satisfaction and objective scores on the Rorschach. Clerks with high scores on the Strong scale show fewer signs of adjustment (Rorschach) than clerks with low interest scores. Clerks possessing interests similar to those of successful office workers tend to have a pattern of Rorschach responses associated with these vocational interests.

Sawatsky (101) examined factors related to turnover among employees in a wire and cable company. Turnover was found to be positively related to absenteeism and negatively related to responsibility. Age, marital status, number of dependents, educational level, earnings, size of department, and skill were unrelated to turnover. Turnover was found to be greater among machine operators than nonmachine operators and in situations in which noise made conversation difficult. It was concluded that employment instability is greater where the work is highly specialized and where the workers have little motility or opportunity for communication. It is suggested that when these conditions occur, there is little opportunity for obtaining social recognition, and the workers are, therefore, less likely to become identified with other members of the group and hence tend to absent themselves more frequently and terminate their employment sooner.

Group Relations

The relatively recent interest on the part of the industrial psychologist in group problems in industry continued to manifest itself during the past year. As a consequence, the boundary lines between industrial psychology and industrial sociology became even less clear and less important. Investigations conducted by members of the one discipline became more frequently referred to by members of the other. This cross fertilization of ideas has not only stimulated conceptual formulations but also resulted in research on more significant problems.

Participation.—In a significant article concerned with the managerial decision-making process, Tannenbaum & Massarik (111) point out that while participation has been stressed in industry and many writers have discussed it, a clear-cut operational definition of the concept has not been

developed nor has a precise set of hypotheses been evolved concerning its dynamics. Broadly speaking, an individual participates in something whenever he takes a part or share in that thing. However, this definition covers all manner of activities such as receipt of wages and the production of goods and services. The type of participation which is of most interest, at least as a management device, is the decision-making process. Tannenbaum & Massarik list the advantages which have been alleged to stem from this type of participation. These include such matters as improved quality of managerial decisions, increased output, reduced turnover, and more peaceful management-labor relations. The authors suggest the following as the psychological conditions of effective participation: the capability of the subordinate of becoming involved in the participational activities, a favorable attitude on the part of the subordinate toward participation, the perception of the relevance of participation on the part of the subordinate, and the capacity of the subordinate to express himself. In addition, there are extra-participational conditions for effective results which include availability of time, rational economics, and intraplant strategy.

By means of the questionnaire method, Armstrong (3) showed that foremen feel that management neither makes its policies clear to them nor accepts their ideas. Furthermore, it appears to foremen that they do not receive the same type of treatment from higher management that they are expected to give to those that they supervise. A marked difference exists between the responsibilities higher management believes are allocated to foremen and the responsibilities foremen assume to be theirs.

A study of the effectiveness of participation of workers in the Library of Congress is reported by Holmes (61). A procedure was established whereby the staff was broken up into small discussion groups which met every other month for a two-hour session on any problems connected with the library or its personnel. The general impression was that this program increased the alertness of operating officials and reduced the differences in attitudes between management and labor. Concretely, it was found that of interviews between employees and supervisors, the proportion sought by employees was reduced from 86 per cent to only 64 per cent. Also, there was almost a complete elimination of interviews based on questions growing out of policy and procedural matters.

Guetzkow (53) reports that when the interrelationships between various committee characteristics and the ratings by the committee's members of its effectiveness are examined, many popular conceptions of what makes a committee effective are contradicted. This investigator found only four factors to be directly related to committee effectiveness: frequency and length of meeting, the preciseness of the agenda, and the type of secretariat service provided the committee.

Labor-management relations.—The participant-observer method was utilized by Dalton (25) in studying unofficial union-management relations. Contrary to the expectations of top union and management officials, there are frequent departures from the provisions of the negotiated contracts.

It was found that these unofficial and often formally unacceptable evasions constitute a great part of the actual functioning relations between management and labor. These informal union-management relations at the plant level appear to rise from a lack of union consciousness among workers, management incohesiveness, and economic and production pressures.

An interesting study by Gottlieb & Kerr (48) illustrates the lack of relationship between evaluations of labor management relations based upon unsystematic opinions and direct measures of worker satisfaction. In a company where both management and the union claimed high emotional acceptance and co-operative attitudes toward each other, it was found that the average score on a job satisfaction questionnaire was not significantly greater than normal expectancy. However, an examination of structure of the attitudes showed a high degree of integration; those workers who were most favorable toward the union were found also to be most favorable toward the company, and workers who were most satisfied with their union stewards were also most satisfied with their supervisors.

In order to analyze the content of labor-management bargaining conferences on the basis of his observations of such conferences, Osterberg (93) attempted to classify the statements made by both parties into eight major categories such as aggression against an opponent and projecting one's own problem to the opposing side. The purpose of this procedure is to enable the investigator to obtain a systematic and integrated picture of the interactional situation in a bargaining conference and one which will be psychologically meaningful. The author gives several examples of the application of this method.

Weschler (122) classified labor mediators as "good" or "poor" on the basis of ratings by other labor mediators. He found that, as compared with the poor mediators, the good mediators were middle-aged rather than either young or old, were better off financially, were more likely to be Democrats than Republicans, were more likely to be Protestants than Catholics or Jews, scored higher on an intelligence test, and were more likely to be impartial than either pro-labor or pro-management. No differences were found in amount of education, occupational history, scores on a test of information concerning labor relations, or scores on the Guilford-Martin personality inventory.

Leadership.—An attempt to formulate the pertinent problems in the important area of leadership was made by Morris & Seeman (92). They indicated that the following questions need to be considered in any study of leadership: Who is the leader? What is defined as leader behavior? How is leader behavior to be described or analyzed? What group and individual factors are significant for leader behavior? Why does the leader behave as he does? What phenomena are concomitant with given kinds of leader behavior? What are the results of leader behavior? What factors serve as conditioners? How are the results of leader behavior evaluated?

Roff (98) presents the results obtained with a 40-item rating scale of leadership ability applied to fairly large numbers of Air Force pilots both

by their supervisors and their subordinates. The ratings assigned to superiors by their subordinates were significantly lower than those assigned to subordinates by their superiors on items relating to interpersonal relationships, but insignificant differences were found on items having to do with flying or combat proficiency. When a comparison was made of the ratings assigned to those officers who were considered to be good combat leaders with those assigned to officers considered to be poor in this quality, items concerned with combat flying proficiency were found to discriminate as well as those concerned with interpersonal relationships.

Browne followed up his earlier investigations into executive leadership in industry with two further studies. In one study (13), he developed a goal achievement index which is based upon the discrepancies between the executives' ratings of importance of various goals and their ratings of the extent to which they believe the organization is achieving these goals. Differences between the indices among members of top management can be taken to signify a lack of adequate communication and the need for corrective action. In order to observe some of the factors involved in leadership, Browne (14) obtained data concerning the sociometric pattern type of relationships existing among 24 executives in one company by asking them to name the individuals with whom they spent the most time. Various groupings emerged which undoubtedly should be considered as idiosyncratic to the organization, therefore probably having little value for generalizations. However, the number of choices were found to bear relationships of a low order with responsibility, authority, and delegation of authority.

In studying 226 careers of management personnel, Dalton (26) found no significant pattern of selection and promotion in terms of age, rate of advancement, occupational experience, or type of education. However, informal processes of selection appeared to be important through use of such factors as religion, ethnic composition, political belief, and participation in accepted organizations.

Goode (47) surveyed some of the literature concerned with the characteristics of effective leadership in working organizations. He noted that while most research workers have concluded that leadership is specific to the situation, other studies have shown some relationships to exist between leadership ability in different situations and between leadership ability and other variables. He lists the characteristics of a successful leader as follows: mental ability, breadth of interests and aptitudes, language facility, maturity, motivation, social orientation, and administrative skills.

Communication.—During the past year many investigators have been concerned with the problem of communication in industry. Beach (7), for example, attempted to set forth the functions and purposes of employee magazines and examined the characteristics of a publication of this kind. Most of the attention in the area of communication, and in the reviewers' opinion much too much, was given to readability and allied problems. Using the Flesch method, Davis & Hopkins (28) computed the readability indices for 71 employee handbooks and found that they averaged at a level of diffi-

cult reading. The Flesch human interest index for these handbooks fell at about the middle of the scale. Farr, Paterson & Stone (35) computed these same indices for 25 management and 25 union publications. The readability index for the management publications averaged difficult and for the union publications fairly difficult. On the basis of the human interest index, the management publications were characterized as mildly interesting and the union publications as dull. A bibliography of pertinent articles concerning the Flesch readability formulae is given by Hotchkiss & Paterson (62).

The concern with readability is more or less academic without information concerning the reading ability of the persons to whom the communications are directed. The only study the reviewers discovered pertinent to this problem was by Colby & Tiffin (21). These investigators administered the Nelson Silent Reading Test to 518 supervisors in seven different manufacturing plants. The mean level of reading ability of these persons was found to be of the order of the 10th grade, somewhat below the level of the communications directed to them as indicated by other investigations. For these supervisors, the reading score was found to bear substantial correlations negatively with age and positively with years of schooling.

Bavelas & Barrett (5) point out that discussions of industrial communication are likely to resolve themselves into considerations of content and technique which will be specific and not provide basic principles with respect to effective industrial communication. These investigators suggest that it is the manager's function to increase the probabilities of achieving the goals of an organization. Thus, the organization becomes a means for increasing certain sets of probabilities, and communication, therefore, should be thought of not merely as a helper but rather as the essence of organized activity out of which all other functions derive.

Cook (22) emphasizes the fact that the importance of communication in industry has been recognized only in the last few years and, to a considerable extent, it is still thought of as the simple process of passing along information. The author stresses communication as a social process and hence shows that problems of communication are closely allied to those of human relations and morale.

Human Engineering

General studies.—Zobel (126) discusses the need for improving the commonly accepted equation of production by including subjective and objective factors which condition labor's productivity. He lists 13 objective factors and 10 subjective factors, most of which are not now quantifiable. He presents several equations, the most reliable of which makes allowances for changes in the aid given labor by machines and changes in the time or effort expended by labor. Coakley (20) presents the theory that the production of automatic machines is subject to variation traceable to the human operator. In an experiment in the hosiery industry, he shows that fabrication of nylon stockings, although accomplished by semi-automatic machines, is significantly affected by variations in the operator.

Perceptual-motor functions.—Previous investigations with respect to the accuracy of making settings on a linear scale by means of a control knob have indicated an optimal ratio of 1 or 2 in. of pointer movement for one complete knob turn. Jenkins, Maas & Rigler (64) point out that the apparatus used in obtaining these findings had a noticeable resistance with extreme ratios, and so they set up an apparatus wherein the friction was the same at all ratios. They found that the optimal ratio between extent of pointer movement and knob turn remained the same. The added friction did not significantly affect time for final adjustment but increased travel time and action potentials in the active forearm.

Ford, Rigler & Dugan (41) examined the accuracy of a pantograph controlled cursor in locating or tracking a spot of light on a dark field and a black spot on a white field and found it two to four times more precise than locating a stimulus by scale reading methods. A reduction ratio of two units of hand movement to one unit of cursor movement appeared to be the optimum.

Accuracy of movement in rotating a selector-switch knob was investigated by Chapanis (17). The subject positioned an indicator knob in reference to certain degree settings in a semicircular vertical plane in front of the body. The settings varied by 10° steps from 0° to 180°. Both the knob and the subject's hand were shielded from view by a curtain. Precision in setting the knob was greater in the vertical and horizontal positions than in angular positions between these points. A negative constant error was found for settings between 0° and 90° and a positive constant error between 90° and 180°. This indicates that the subject positioned the knob somewhat more vertically than the setting called for. Settings between 0° and 90° were made with more precision by the left hand and settings between 90° and 180° with more precision by the right hand. In a second experiment, Chapanis (18) studied the accuracy of estimation of the position of an indicator knob. The experimenter set the position of the knob, and the subject then felt the knob with his preferred hand and made a judgment concerning its angular position in terms of degree settings from 0° to 180°. A visual guide in the form of a protractor was mounted in front of the subject. The knob and the subject's hand were shielded from view. The precision of estimating the position of the knob was lower than the precision of actually setting the knob. The constant errors were reversed in sign from those obtained in the first experiment. The variable errors were greater for estimates than for settings of the indicator knob.

Mackworth (80) reports a large number of experimental studies completed in England during World War II on psychological factors conditioning the detection of either faint visual or faint auditory signals. Although most of the material would be classed in the area of sensory psychology, some of the findings are of interest to the worker in human engineering. In the studies in vision, the subject watched a pointer moving like the hand of a clock with stepwise motion, i.e., a pointer which moved suddenly to a new position once each second. At certain times, the pointer would move

the equivalent of two steps, and the task of the subject was to detect when this occurred. Another test was a synthetic radar screen with a scanning light on which the appearance of a small pin point of light was to be detected by the subject. Auditory tests involved detecting a sound of a constant duration which occurred randomly among repetitions of a sound of shorter duration. In general, the accuracy of the subject's responses dropped rapidly after half an hour of continuous work. Decline was prevented by alternating the task with another type of work. Supplying knowledge of results tended also to prevent deterioration in accuracy during the 2-hr. test. When given benzedrine, the subject maintained a high level of accuracy for the entire test period.

A study on visual interpolation for various interval sizes was conducted by Miller (90). Size of the interval and of the markers limiting it, and biases related to the particular location in the interval of the point being judged, contributed less to the accuracy than did individual differences. Biesele (9) investigated the effect of variations in the brightness ratio between task and surround upon the speed of vision (recognition of the gap in Landolt rings). Preliminary experiments indicated that varying the ratio produced no effect in a practice period of 1 hr., but a detrimental effect for some ratios was obtained in a 4-hr. period. Ratios of over one to one appeared to have a marked detrimental effect. Allphin (1) reports the changes in tension level, as measured through muscle action potentials, which accompanied changes in intensity of illumination during the performance of industrial tasks. Tension level was found to increase as intensity was decreased. Stern (107) studied the speed of composition of linotype operators as a function of the copy, whether printed in upper case or lower case. No significant differences were found. An investigation of the ability to discriminate the firmness of deformable materials was conducted by Harper (54). This is a response demanded of cheese inspectors in grading the product in terms of its firmness. He devised an equation for expressing the sensitivity function and reported variation in sensitivity over time for several subjects.

Reaction time.—Lazarus (76) made a study of the amount of error found in the continuous and the snap-back methods of using the stop watch in measuring time elements. Reactions were recorded for 50 industrial engineers on time elements varying from .04 to .39 min. in length. In half of the experiment, the limiting signals were auditory in nature and in the other half visual. Mean errors for visual signals were, for the continuous method, .000097 min. and for the snap-back method, −.0008 min. In auditory presentation, neither method gave errors significantly different from zero.

Fatigue.—Griffith *et al.* (52) had 379 workers report which period of the morning and which period of the afternoon they felt most tired and most rested. Maximal subjective fatigue was reported for the last hour of the morning and afternoon, and maximal restfulness feeling for the second hour of the morning and afternoon. A study of the effects on certain complex tasks of fatigue from flying is reported by Welford & Brown (121). One problem involved matching terminals on a resistance box with terminals on a

circuit diagram. A second problem was a psychomotor plotting task. Results show that impairment occurs if the task is met for the first time when in a fatigued state (after flight). If the task is given for the first time before flight, no impairment is demonstrated in the fatigued state after flight. Difficult trips sometimes gave greater impairment than easy trips. The reviewers suggest that better control over practice effects and examination of more cases must be accomplished before the conclusions can be confidently accepted.

Physiological conditions.—In the article by Mackworth (80), experiments are described on the effects on human performance of abnormal atmospheric environment. He reports that there is a critical point on the atmospheric temperature scale above which air acclimatized individuals cannot work effectively whether the work is physical or mental. The temperature region is between dry bulb/wet bulb readings of 90/80°F. and 95/85°F. with air movement of 100 ft. per min. The accuracy of highly skilled workers is less affected than that of workers with less skill. Regardless of skill, if the amount of work done is the criterion, and output is restricted, then the worker who suffers the fastest deterioration is the one who is putting the greatest effort into his response. In three entirely different performance tests (mental, psychomotor, and semiskilled heavy), the index of deterioration was shown to bear a logarithmic relationship to the atmospheric temperature. Smith (105, 106) studied the relation between quantity and quality of output of women workers and the physiological changes incident to the menstrual cycle. No relation was found between menstruation and either activity level on the job or amount of production.

Accidents

Kerr (70) studied the relationships between accident frequency and severity and 40 other variables in 53 departments of an electronics factory. Accident frequency was found to be associated with low intracompany transfer mobility, small percentage of employees who are female and on salary, low promotion probability, and high noise level. Accident severity was found to be associated with large percentage of employees who are male, low promotion probability, infrequency and kind of employee suggestions, and high average employee age level and employee tenure. As a result of these findings, the author suggests that high accident frequency and severity are positively related to an attitude of indifference to work and negatively to alertness. The key to accident reduction, therefore, is a change in the total psychological frame of reference in which the individual works, with rewards given for alertness, constructive suggestions, and advising co-workers on safe methods of work.

Harris (55) compared 25 industrial accident repeaters with 25 control subjects matched with respect to sex, job, age, and experience, on biographical data, Bernreuter-type personality items, the multiple-choice Rorschach, and the Rosenzweig Picture-Frustration Study. No significant differences were found between the scores of the two groups. This study is another in

the fruitless series seeking to discover evidences of a general propensity to be involved in accidents under all circumstances. The notion of accident proneness as a general trait clearly seems invalid, and hence, the comparison of the test scores and the accident rates of groups heterogeneous with respect to occupation would be expected to yield negative results.

LITERATURE CITED

1. Allphin, W., *Illum. Eng.*, **45,** 562 (1950)
2. Ammons, R. B., Butler, M. N., and Herzig, S. A., *J. Applied Psychol.*, **34,** 198–205 (1950)
3. Armstrong, T. O., *Personnel*, **27,** 70–75 (1950)
4. Ash, P., *Public Personnel Rev.*, **11,** 130–38 (1950)
5. Bavelas, A., and Barrett, D., *Personnel*, **27,** 366–71 (1951)
6. Bayroff, A. G., and Burke, J. H., *Personnel Psychol.*, **3,** 461–65 (1950)
7. Beach, B., *Personnel J.*, **29,** 216–20 (1950)
8. Bentley, C. H., *Public Personnel Rev.*, **11,** 119–25 (1950)
9. Biesele, R. L., Jr., *Illum. Eng.*, **45,** 555 (1950)
10. Bittner, R. H., and Rundquist, E. A., *J. Applied Psychol.*, **34,** 171–77 (1950)
11. Brogden, H. E., and Taylor, E. K., *Educ. Psychol. Measurement*, **10,** 159–86 (1950)
12. Brogden, H. E., and Taylor, E. K., *Personnel Psychol.*, **3,** 133–54 (1950)
13. Browne, C. G., *J. Applied Psychol.*, **34,** 82–87 (1950)
14. Browne, C. G., *J. Applied Psychol.*, **35,** 34–37 (1951)
15. Canter, R. R., Jr., *J. Applied Psychol.*, **35,** 38–45 (1951)
16. Carr, E. R., and Rothe, H. F., *J. Applied Psychol.*, **34,** 178–81 (1950)
17. Chapanis, A., *J. Psychol.*, **31,** 51–64 (1951)
18. Chapanis, A., *J. Psychol.*, **31,** 65–71 (1951)
19. Christensen, J. M., *Personnel Psychol.*, **3,** 361–68 (1950)
20. Coakley, J. D., *Personnel Psychol.*, **3,** 401–11 (1950)
21. Colby, A. N., and Tiffin, J., *Personnel*, **27,** 156–58 (1950)
22. Cook, P. H., *Occupational Psychol.*, **25,** 1–14 (1951)
23. Cottle, W. C., *Psychometrika*, **15,** 25–47 (1950)
24. Cross, O. H., *Educ. Psychol. Measurement*, **10,** 271–77 (1950)
25. Dalton, M., *Am. Sociol. Rev.*, **25,** 611–19 (1950)
26. Dalton, M., *Am. J. Sociol.*, **56,** 407–15 (1951)
27. Daniels, H. W., and Otis, J. L., *Personnel Psychol.*, **3,** 425–44 (1950)
28. Davis, K., and Hopkins, O., *Personnel Psychol.*, **3,** 317–26 (1950)
29. Davis, M. K., and Tiffin, J., *J. Applied Psychol.*, **34,** 225–28 (1950)
30. Degan, J. W., *A Re-Analysis of the Army Air Force Battery of Mechanical Tests* (Psychometric Lab., Univ. of Chicago, No. 58, 13 pp., 1950)
31. Douglas, A. G., *Public Admin. Rev.*, **10,** 78–86 (1950)
32. DuBois, P. H., and Watson, R. I., *J. Applied Psychol.*, **34,** 90–95 (1950)
33. Dulsky, S. G., and Krout, M. H., *Personnel Psychol.*, **3,** 345–51 (1950)
34. Ely, J. H., Kephart, N. C., and Tiffin, J., *J. Applied Psychol.*, **34,** 232–34 (1950)
35. Farr, J. N., Paterson, D. G., and Stone, C. H., *Ind. Labor Relations Rev.*, **4,** 88–91 (1950)
36. Ferguson, L. W., *Personnel Psychol.*, **3,** 193–216 (1950)
37. Fields, H., *Public Personnel Rev.*, **11,** 139–46 (1950)
38. Fiske, D. W., *Personnel Psychol.*, **4,** 93–98 (1951)

39. Flanagan, J. C., in *Current Trends in Industrial Psychology*, 32–54 (Dennis, W., Ed., Univ. of Pittsburgh Press, 198 pp., 1949)
40. Ford, A. H., *J. Applied Psychol.*, **34**, 186–89 (1950)
41. Ford, A., Rigler, D., and Dugan, G. E., *J. Applied Psychol.*, **34**, 429–33 (1950)
42. Freyer, D. H., *Personnel Psychol.*, **4**, 19–37 (1951)
43. Freyer, D. H., and Edgerton, H. A., *Personnel Psychol.*, **3**, 261–84 (1950)
44. Ghiselli, E. E., and Brown, C. W., *J. Applied Psychol.*, **35**, 23–24 (1951)
45. Gilbert, C., *J. Applied Psychol.*, **34**, 394–96 (1950)
46. Gomberg, W., *J. Applied Psychol.*, **35**, 1–7 (1951)
47. Goode, C. E., *Personnel*, **27**, 342–50 (1951)
48. Gottlieb, B., and Kerr, W. A., *Personnel Psychol.*, **3**, 345–53 (1950)
49. Gray, J. S., *J. Applied Psychol.*, **34**, 378–80 (1950)
50. Gray, J. S., *J. Applied Psychol.*, **35**, 8–10 (1951)
51. Gray, J. S., and Jones, M. C., *J. Applied Psychol.*, **35**, 11–14 (1951)
52. Griffith, J. W., Kerr, W. A., Mayo, T. B., Jr., and Topal, J. R., *J. Applied Psychol.*, **34**, 163–66 (1950)
53. Guetzkow, H., *Public Admin. Rev.*, **10**, 190–96 (1950)
54. Harper, R., *Occupational Psychol.*, **24**, 230–38 (1950)
55. Harris, F. J., *Personnel Psychol.*, **3**, 455–59 (1950)
56. Hay, E. N., *J. Applied Psychol.*, **34**, 102–4 (1950)
57. Hay, E. N., *J. Applied Psychol.*, **34**, 153–58 (1950)
58. Henry, W. E., and Moore, H. B., *Advanced Management*, **15**(11), 16–20 (1950)
59. Holmes, F. J., *Personnel Psychol.*, **3**, 57–69 (1950)
60. Holmes, F. J., *Personnel Psychol.*, **3**, 217–20 (1950)
61. Holmes, R. M., Jr., *Personnel Admin.*, **12**, 6–7, 18 (1950)
62. Hotchkiss, S. N., and Paterson, D. G., *Personnel Psychol.*, **3**, 327–44 (1950)
63. Humm, D. G., and Humm, K. A., *J. Psychol.*, **30**, 63–75 (1950)
64. Jenkins, W. L., Maas, L. O., and Rigler, D., *J. Applied Psychol.*, **34**, 434–39 (1950)
65. Jones, F. N., and Smith, C. J., *J. Applied Psychol.*, **34**, 313–15 (1950)
66. Jones, M. H., *J. Applied Psychol.*, **34**, 219–24 (1950)
67. Kates, S. L., *J. Applied Psychol.*, **34**, 249–54 (1950)
68. Kates, S. L., *Psychol. Monographs*, **64**(3), 1–35 (1950)
69. Kephart, N. C., and Mason, J. M., *J. Applied Psychol.*, **34**, 423–28 (1950)
70. Kerr, W. A., *J. Applied Psychol.*, **34**, 167–70 (1950)
71. Knight, J. F., *Advanced Management*, **15**, 21–22 (1950)
72. Kuder, G. F., *Personnel Psychol.*, **3**, 155–67 (1950)
73. Lattin, G. W., *Occupations*, **29**, 36–39 (1950)
74. Lawshe, C. H., *Personnel Psychol.*, **4**, 63–92 (1951)
75. Lawshe, C. H., and Guion, R. M., *Personnel Psychol.*, **4**, 3–17 (1951)
76. Lazarus, I. P., *Advanced Management*, **15**(5), 15–16 (1950)
77. Lennon, V. A., and Shapiro, R. B., *Personnel*, **27**, 43–46 (1950)
78. Lott, C. S., and Boddie, J., *Personnel Admin.*, **12**(5), 11–15 (1950)
79. Mace, C. A., *Occup. Psychol.*, **24**, 239–44 (1950)
80. Mackworth, N. H., *Researches on the Measurement of Human Performance* (Brit. Med Research Council, Special Rept. Series No. 268, 153 pp., 1950)
81. Mandell, M. M., *Personnel Psychol.*, **3**, 179–85 (1950)
82. Mandell, M. M., *Personnel*, **27**, 215–16 (1950)
83. Mandell, M. M., *J. Applied Psychol.*, **34**, 145–47 (1950)
84. Martin, G. C., *J. Applied Psychol.*, **35**, 20–22 (1951)

85. Maslow, A. P., *Public Personnel Rev.*, **11,** 96–99 (1950)
86. McCormick, E. J., and Winstanley, N. B., *Personnel*, **27,** 144–46 (1950)
87. Medical Research Council: Building Research Unit, *Occupational Psychol.*, **24,** 96–104 (1950)
88. Meyer, C. A., *Public Personnel Rev.*, **11,** 147–54 (1950)
89. Meyer, H. H., *J. Applied Psychol.*, **35,** 46–49 (1951)
90. Miller, H. K., Jr., *J. Applied Psychol.*, **34,** 367–70 (1950)
91. Mitchell, M. B., and Rothe, H. F., *J. Applied Psychol.*, **34,** 329–32 (1950)
92. Morris, R. T., and Seeman, M., *Am. J. Sociol.*, **56,** 149–55 (1950)
93. Osterberg, W. H., *Personnel Psychol.*, **3,** 169–78 (1950)
94. Owens, W. A., *J. Applied Psychol.*, **34,** 295–99 (1950)
95. Page, H. E., *J. Applied Psychol.*, **34,** 306–8 (1950)
96. Poruben, A., Jr., *J. Applied Psychol.*, **34,** 159–62 (1950)
97. Poruben, A., Jr., *J. Psychol.*, **29,** 133–55 (1950)
98. Roff, M., *J. Psychol.*, **30,** 229–39 (1950)
99. Rothe, H. F., *J. Applied Psychol.*, **34,** 98–101 (1950)
100. Rothe, H. F., *Personnel Psychol.*, **3,** 187–92 (1950)
101. Sawatsky, J. C., *Can. J. Psychol.*, **5,** 29–38 (1951)
102. Schmidt, R. C., *Personnel J.*, **29,** 226–30 (1950)
103. Seidman, J., London, J., and Karsh, B., *Am. J. Sociol.*, **56,** 229–37 (1950)
104. Simon, L. M., and Levitt, E. A., *Occupations*, **29,** 23–25 (1950)
105. Smith, A. J., *J. Applied Psychol.*, **34,** 1–5 (1950)
106. Smith, A. J., *J. Applied Psychol.*, **34,** 148–52 (1950)
107. Stern, B., *J. Applied Psychol.*, **34,** 351–54 (1950)
108. Stockford, L. O., and Kunze, K. R., *Personnel*, **27,** 2–15 (1950)
109. Strong, E. K., Jr., *J. Applied Psychol.*, **35,** 50–56 (1951)
110. Super, D. E., *Occupations*, **29,** 174–76 (1950)
111. Tannenbaum, R., and Massarik, F., *Participation by Subordinates in the Managerial Decision-Making Process* (Inst. Ind. Relations, Univ. of Calif., Los Angeles, No. 14, 11 pp., 1950)
112. Taylor, E. K., and Wherry, R. J., *Personnel Psychol.*, **4,** 39–47 (1951)
113. Thorndike, R. L., *Psychometrika*, **15,** 215–35 (1950)
114. Thurstone, L. L., *Individual and Group Tests of Mechanical Aptitude* (Psychometric Lab., Univ. of Chicago, No. 57, 7 pp., 1950)
115. Thurstone, L. L., *An Analysis of Mechanical Aptitude* (Psychometric Lab., Univ. of Chicago, No. 62, 26 pp., 1951)
116. Tupes, E. C., *Psychol. Monographs*, **64**(11), 1–24 (1950)
117. Uhrbrock, R. S., *Personnel Psychol.*, **3,** 285–316 (1950)
118. Vernon, P. E., *Occupational Psychol.*, **24,** 75–95 (1950)
119. Wagner, R. F., *Development of Standardized Procedures for Defining the Requirements of Aircrew Jobs in Terms of Testable Traits* (School of Aviation Med., United States Air Force Project Rept. No. 21-29-010, 94 pp., 1951)
120. Webster, E. C., Winn, A., and Oliver, J. A., *Personnel*, **27,** 414–23 (1951)
121. Welford, A. T., and Brown, R. A., *Brit. J. Psychol.*, **45,** 195–211 (1950)
122. Weschler, I. R., *Personnel Psychol.*, **3,** 113–32 (1950)
123. West, E. D., *Occupations*, **29,** 438–40 (1951)
124. Wolff, W. M., and North, A. J., *J. Applied Psychol.*, **35,** 25–29 (1951)
125. Worbois, G. M., *J. Applied Psychol.*, **35,** 15–19 (1951)
126. Zobel, S. P., *J. Am. Statistical Assoc.*, **45,** 218–24 (1950)

COMPARATIVE AND PHYSIOLOGICAL PSYCHOLOGY[1]

By Henry W. Nissen and Josephine Semmes
Yale University, New Haven, Connecticut
and
Yerkes Laboratories of Primate Biology, Inc., Orange Park, Florida

The fields of both comparative and physiological psychology are so broad that, for authors of this chapter, the problem of choice of topics is particularly acute. A major consideration influencing our selection has been the attempt to bring to attention points of view and areas of investigation which have not come within the compass of treatment in previous volumes of the series.

The Status of Comparative Psychology

The lack of adequate methods and the absence of an established framework for comparative psychology have been deplored by the authors of this chapter in the two preceding volumes of the *Annual Review of Psychology*. More recently, Beach (15) has pointed with regret and with substantiating statistical evidence at the decline, in American psychology, of the comparative approach and increasing restriction "to a small number of species and a small number of behavior patterns." The situation has not changed radically during the period here under consideration. It is true that an impressive fraction of basic research published in American journals has been done with animal subjects, but most of it has been with the rat and has been directed at the analysis of learning rather than, in any immediate fashion, at questions of evolutionary origins and phylogenetic development.

However, the outlook for comparative psychology is not perhaps as black as it seems. The steadily growing body of information derived from our specialized rodent studies will eventually be highly useful for, and will be incorporated into, comparative psychology. Rat psychology is producing behavioral facts, and it is clear that before we can make comparisons, we must have the facts to compare. Furthermore, a "phylogeny of behavior," when it comes, must be in terms of behavioral dimensions, laws, or mechanisms which can be formulated independently of the species-variable structures through which they operate. The fact that work with the white rat is highly analytical makes it more, rather than less valuable for comparative purposes.

Secondly, the alleged narrowness of the American approach to animal behavior has been complemented by the development of a broadly naturalistic approach in Europe. This movement began at least 15 years ago, but so far has not received the attention in America that it deserves. The resurgence

[1] The survey of the literature pertaining to this review was concluded in May, 1951.

of interest in behavior of the animal as a whole and in the direct application to behavioral data of the framework provided by the theory of evolution had its *Anlagen* in the careful observations and sagacious interpretations of such investigators as Craig, Heinroth, and von Uexküll, among others. It was mainly Lorenz, however, who provided the theoretical integration and stimulated the methodological advances which consolidated the trend and gave direction to the movement.

In briefest outline, the work of the Lorenz-influenced group is characterized by an immediate concern with the reciprocal implications between behavior and the problem of phylogenetic relations, by its attempt to make inclusive behavior-inventories at all levels of evolutionary development, by emphasis on the full range of behavior in relation to the ecological conditions in which those behaviors have evolved rather than on the few responses possible in highly simplified experimental situations, and by the ingenious fusion of experimental with field-observational techniques. Whether arrived at inductively from the data, or in part from theoretical preconceptions, the behavioral profiles which these investigators present give preponderant weight to innately determined components. The presence of learned elements in any prolonged behavior-sequence is stressed, but their importance is subordinated. Whereas American psychologists were quick to bring their problems into the laboratory for quantitative analysis, the neo-naturalists (as we may name them for convenience) have extended and refined the original approach, as exemplified in early studies of the adequate stimulus for innate behavior and the waning of instincts by workers such as Whitman, Yerkes, Watson, Lashley, and Stone.

Analysis of Instinctive Behavior

The writings of the neo-naturalists during the period here under consideration have been extensive. Nine publications (5, 10, 11, 32, 57, 91, 143 to 145) which have appeared in English constitute a representative sample, although not an adequate summary, of the views and mode of work of this group; they are reviewed in this section. The papers by Armstrong (5) and Thorpe (143) are peripheral but help to round out the picture.

Lorenz (91) discusses "the comparative method in studying innate behavior patterns." He points out that comparative ethology (bionomics, ecology, or in the sociological sense, customs and manners) is based on a particulate process, variously called *Erbkoordinationen*, *Instinktbewegungen*, or "endogenous movements." An endogenous movement is a rather simple movement pattern—usually not as simple as an "unconditioned reaction," however—common to all members of the species. Endogenous movements are the invariable core of instinctive behavior; the latter includes also (*a*) releasing mechanisms, typically activated by innately determined perceptions of simple stimulus-patterns or by early imprinting, and (*b*) taxes, kineses, and interlaced learned elements which steer or direct the activity according to the particulars of the situation. Among the "methods peculiar

to comparative ethology," Lorenz stresses the search for phyletic homologies of innate behavior patterns. The "keeping" of animals, not in the narrow confines of a cage, but more in the manner of pets, is recommended as a widely useful approach to the analysis of their behavior. The mistakes of instinctive action, when environmental conditions are slightly altered, give the clues to its determinants. Tinbergen (144) presents a conceptualization of the hierarchical nervous organization underlying instinctive behavior. At the top level is the center for the dominant instinct of the moment, presumably activated by physiological or homeostatic processes, which elicits "appetitive" or searching behavior. If the latter leads to the relevant external stimuli, the internal releasing mechanism removes the block to further endogenous movements belonging to this instinct. Each successive level in the sequence may require additional searching and always requires adequate external stimulation to activate the internal releasing mechanism leading to the next level. The motor side of the behavior is represented as a fanning out from centers controlling major effectors to those controlling individual muscles and finally motor units.

Social releasers or *Auslöser* are patterns of stimulation emanating from one individual and eliciting a consistent response in another. The pattern may be a movement or a structure; often it is a combination in which the movement emphasizes or displays a structural pecularity (an "ornament"). Baerends (10) discusses the evolution of such social releasers, drawing many illustrations from his detailed studies (11) of various Cichlid fishes in which some 40 movement-patterns of the third level (see preceding paragraph) are identified and subsumed under eight "centers" of the second order, all of these belonging to one of two instincts or "moods" (*Stimmungen*) of the first order. Indicative evidence is presented to show that homologous movements in two closely related species may have a releasive function (on other individuals) in one but not in the other. If this function has survival value, it will tend to be selected, in the Darwinian sense. Such a movement often becomes formalized or ritualized for its communicative effect. Extending the concept of social releasers to interspecies relations, Baerends (10) suggests that many protective structures and behaviors have had an analogous evolutionary development: insofar as they are adapted to avoid the release of attack by enemies (e.g., protective coloration, death-feigning) or to release behaviors incompatible with attack (e.g., repelling odors, frightening sounds), they would tend to be genetically established.

Intraspecies "communication" (in the sense that the movement structure pattern produced by one individual elicits a predictable response in others) may be derived from incipient activities, i.e., "intention movements." Because many instinctive acts begin with locomotion towards or away from an object, Daanje (32) suggests that most intention movements are low intensity forms of locomotion. In birds, hopping and walking involve the body, head, neck, wings, and tail, as well as the feet, giving many possibilities for the development of intention movements which may act as a releasing

pattern for other individuals. The "sparking-over" [Tinbergen (145a)] of movements belonging to one instinct to the context and use of another instinct is termed *Uebersprungsbewegung* or "displacement activity." Armstrong (5) points out that displacement activities arise when there is conflict or thwarting of drives.

> A species which is able to modify its behavior to suit changed circumstances by means of displacements, rather than by the evolution of *ad hoc* modifications starting from scratch, will have an advantage over other species. . . . a displacement-prone species will be more adaptable and consequently more successful.

Thorpe's penetrating discussion (143) of the "concepts of learning and their relation to those of instinct" lists five "grades" of learning: habituation, conditioning, trial and error, insight, and imprinting. He says, "Probably all of them can be found in one form or other among the insects." Insight is considered a rapid form of learning "which shows in a particularly clear manner something of the true nature of the process which is the basis of the other types." Thorpe believes that learning is involved in all perception of patterns and that such learning must therefore be phylogenetically prior to instinct as well as an essential component of even rigid instincts.

Tinbergen & Perdeck (145) analyze the stimulus situation releasing the begging response of newly hatched herring gulls. The method employed is the *Attrapenversuch*, that is, the use of dummies or artificial models which isolate or emphasize certain aspects of the natural object under investigation. The gull chick pecks at the bill of the parent bird in an invariable fashion (endogenous movement). By presenting artificial beaked heads they demonstrated that it was the beak alone which brought about the response; varying the form, size and color of the head was without effect. Then artificial beaks which varied in many visual aspects were presented, frequency and promptness of the responses being recorded. Many thousands of observations on many chicks tested in their nests permitted a quantitative assessment of seven characteristics of the beak contributing to elicitation of the response: its movement, form, nearness, lowness, angle to the vertical, a patch (preferably red) near the ventral surface of the tip, and an object projecting from the beak. By accentuating some aspects, e.g., having the artificial beak point down almost vertically, the eliciting value could be carried beyond that of the natural object—a "super-dummy." A similar technique was used by Hartley (57) in a study of interspecific recognition. The effectiveness of owl-models differing in size, proportions, evenness of contour, and patterning was tested by "mobbing reactions" on the part of birds which are the natural prey of the owl. It was found that for many species of passerine birds, the same owl-characters were the most effective ones.

Phylogenetically Oriented Studies: Innate Behavior

Mammals.—By intent and organization, the volume by Ford & Beach (39) is an exemplary contribution to comparative psychology. In it are exhibited, side by side, the patterns of sexual behavior to be found in some

190 human societies and in a variety of infrahuman animals. The fact that the descriptive juxtapositions do not often permit unambiguous identification of detailed homologies or of the factors involved in phylogenetic modifications of a given pattern is clearly attributable to incompleteness of the requisite data. The great range of sexual behavior made possible by man's inherited mechanisms suggests the strategic value of detailed comparisons with other primate species.

Schneider (122) reports at length observations of a chimpanzee infant during the first 10 months of life. The ape was raised by human caretakers under conditions designed to minimize effects of training and experience. The author emphasizes that most of the activities occurring during this early period are innate; similarities to human behavior and their accelerated appearance in the ape are pointed out.

Social organization of the field mouse (*Aehrenmaus*) is described by Eibl-Eibesfeldt (36). He brought some captured specimens to the area where he was living, provided food supplies, and observed them as they multiplied and established themselves in the territory. The mice are organized into superfamilies, with a large male usually the leader. The territorial limits of the superfamily are marked with scent and are vigorously defended. The territory is divided into nesting, hiding, eating, and resting places, with special areas for defecation and urination. Mating is initiated by olfactory stimuli from the female. A female which is not ready to mate assumes the "humility posture." Many matings occur without ejaculation; individual differences in mating behavior among the males are considerable. Grooming is an innate pattern, and social or heterogrooming is common. Ontogenetic sequences in the appearance of various behavior patterns are described in detail. Sucking movements often occur *in vacuo* (*Leerlaufreaktion*). The mice are omnivorous; what is edible is learned by experience rather than determined by inborn perceptual schemata.

Steiniger (137) made a similar study of the brown Norway rat. In establishing a new social organization, the female mates only with a male superior to herself in dominance-ranking. A large, strong female may thus be left without a mate. Each pair dominates a certain territory which it possessively defends. The offspring of the pair, and their offspring, remain in the territory; and so a herd (*Rudel*) is formed. Within the herd there is no dominance ranking, and often the younger animals, because of greater activity, have advantages over the larger, older animals. Defense of territory, getting food, and care of the young are community enterprises. During estrus, the females mate with all males of the herd. "Thus the rat herd has the character of a super-individual unity—which reminds one in many respects of relations within the social insects." Peculiarities of behavior, not constant for the species, may occur in the herds of certain geographical localities. When there is an abundance of food, they hoard much more than they can eat and store it in special "expandable" structures used only for this purpose. This observation is consistent with studies of other hoarding species, including

birds as well as mammals, in suggesting at least a partial dissociation between hunting, killing, eating, and hoarding activities.

The method of making comprehensive behavior inventories is illustrated by several studies. Lindemann (88) reports extensive observations and tests of two lynxes, raised from infancy in isolation from other members of their species. Movements and activities occurring without specific training or experience were catalogued. Sensory capacities were investigated; good visual form discrimination was indicated, but the olfactory sense was either poor or not exploited. In three-choice delayed reaction tests they were successful at delays up to 30 min. Instrumental stick-using problems were solved, in some cases more quickly and with better retention than by a chimpanzee.

A male badger, born in captivity, was raised by Eibl-Eibesfeldt (35) under relatively free and natural conditions. A full descriptive account of the sequential appearance of various behaviors is given. Until weaning (from the bottle) at 110 days, he was active only during the day; thereafter, he suddenly became nocturnal. Coincident with this change, stimuli or situations which previously, and now only during late afternoon and evening, evoked "confident" investigatory and playful behavior, produced slyness, fear, and flight when they occurred during the day. The badger has rather poor vision and depends predominantly on olfactory and auditory cues. He has a scent-producing organ with which he marks his territory, captured prey, and the shoes of his human friends; this organ became functional within a week after weaning, long before sexual maturity. The author presents a lengthy discussion of the functions and evolutionary development of play behavior in reference to instincts, releasing stimuli, and ritualization. [In this connection see also Lorenz (90, 91).]

The behavior of adult caged foxes and the development of red- and silver-foxes isolated from their kind at early ages were observed by Seitz (125). If the young were isolated from older animals before the twenty-fourth day of life, without exception they were tame and they directed various social responses at their human contacts. If the separation was made after the age of 36 days, however, the young foxes were extremely timid and defended themselves against handling. In some cases, this timidity was gradually overcome. The observations are interpreted as indicating that the fox does not have an inborn perceptual schema of the species-companion, but that the perception is acquired through early imprinting. However, when the early separated animal is put with a species-companion, it behaves normally after a short initial period of uncertainty. The author lists some 40 innately determined behaviors, 17 of them social, which occur sequentially within fairly narrow age-ranges by the age of 54 days. "Critical periods" in the development of dogs are discussed by Scott & Marston (123). In noncompetitive approach to a goal, dogs ran significantly faster in pairs than singly, if the members of the pair were familiar with each other (124).

Birds.—Guhl (51) determined the dominance rankings within a relatively

large flock of hens (domestic fowl) and used frequency of crouching in the presence of a cock as a measure of receptivity. The results are interpreted as showing that "the habit of dominance exerts a suppressive influence on the mating behavior of hens." Levy (84) describes the initial hostility of a group of hens towards the stranger in their midst, followed by a gradual process of acceptance.

According to Löhrl (89), crows have two distinct vocalizations in the presence of danger. The "alarm cry" is given when an enemy approaches from the air, and it causes the other crows to fly upwards so as to get above the predator. The other vocalization, usually given when an enemy approaches on the ground, changes to the "alarm cry" only in case of extreme excitement. Seitz (126) describes 13 different releasing situations and their differentiable vocalization-patterns in the plover. This author also describes the sequential interlocking of innate behaviors of the male and female of the plover in courtship and mating.

The adult common curlew has no distinct markings which could provide the uncomplicated stimulus-pattern required, according to Lorenz, for an innate releasing mechanism; nevertheless, Seitz (127) points out, the artificially incubated curlew does not attach itself to the human caretaker by imprinting as does the gray goose. The author concludes that in the releasing mechanism of the curlew, a simple pattern of vocalization must play a major role.

Räber (114) made a detailed analysis of predatory behavior and its releasing mechanisms in owls. For eliciting attack on other birds, a still object which approximates avian size, form, and proportions, provides the necessary and sufficient stimulating conditions. In taking beetles, movement of the limbs on an oval body which is or is not moving, is necessary. An unfledged owl catches no prey and shows fear of living mice. From fledging to moulting season, they attack any moving object. After having caught and killed a few mice, they lose their previous interest in such moving objects as a rag and a ball. A young owl which did not see its first living mouse until after moulting (age, three months) made threatening movements, but did not strike a live mouse, even after fasting for two days.

Fish.—Aronson (6) found that the spawning frequency of a chichlid fish depends on a complex of internal and exteroceptive stimulation in which the presence of a second fish (behind a glass partition) plays a major role. Intact males and females and castrated males are about equally effective as a stimulus, castrated females less so.

Insects.—A good summary in English of von Frisch's work on the vision, chemical senses, and communicative behavior of bees is now available (42). Further details concerning the mechanism involved in the use of light-polarization are given in another publication (43). Hein (59) confirms the observations of Baltzer & Tschumi that some types of bees indicate direction even when the food-source is nearby. (The bees observed by von Frisch communicated direction only if the source was 25 m. or more away.) The

Rucktanz (pull- or jerk-dance) consists of a wiggling locomotion in the correct direction (translated to angular deviation from the vertical) which may occur when the food-source is as little as 2 m. from the hive. At somewhat greater distances, the total locomotory pattern becomes repetitive and regularized into the form of a sickle (*Sicheltanz*) with the direction-giving wiggles occurring near the two end points of the sickle. At still greater distances, the sickel-form gradually goes over to the figure-eight dance as described by von Frisch.

The formation of new societies of termites by the phenomenon of sociotomy—fission of the society—is described by Grassé & Noirot (49). At this time, the specialization of function of the various castes, all of which are represented in the process, breaks down, and behaviors occur which are never seen at other times. Jacobs (65) suggests that the sound-productions of grasshoppers have been derived phylogenetically as displacement activities from defense behavior and this, in turn, from locomotory movements. The vocalizations are now used in the service of quite unrelated instincts.

Phylogenetically Oriented Studies: Learning and Perception

The coupling of learning and perception is perhaps not as unusual today as it would have been 10 years ago. Until recently, these two topics represented unrelated, if not opposing interests and views in psychology. It would appear that we are now at a confluence of emphases on these two areas, with consequent interest in the relations between them. Thorpe (143), as we have noted above, sees in perception and insight the essence of the learning process. Nissen (107) assigns to perception—the question of what is and what may become a stimulus—the primary role in behavior determination and gives to the process of connecting a preferential response to a given percept only secondary importance. In animal studies, determination of the role of experience by experimental restriction and control of stimulation is a currently popular approach.

Hess (60) raised a control group of chicks with "normal" illumination from above and an experimental group with illumination from below. The results indicate that learning is involved in development of response to cues of depth or of "objectness," but suggest a greater readiness for the "normal" or "natural" stimulus pattern.

Schiller (121) investigated the problem of whether a "natural" or congruent response is learned more readily than an incongruent response. A two-choice detour situation was used with cats. The animal saw one or the other compartment (in irregular sequence) being baited with food and then (*a*) congruently and consistently found the food on that side, or (*b*) incongruently but consistently on the alternative side. Problems (*a*) and (*b*) were learned in about the same number of trials. The time-scores, however, were higher for the incongruent problem, and the author interprets this "as a consequence of emotional strain. . . . The frustration of the incongruous

situation, at variance with the general experience of the animal, delays his learning."

One-trial learning in rats when a strong electric shock was used as the unconditional stimulus is described by Hudson (64). Skard (130) compared the learning of rats in an alley maze with that of human subjects on a stylus maze of the same pattern. The latter learned about one-third faster. In a modified repetition of the well-known Tolman & Honzik experiment, Brown & Gentry (25) found that only one of eight rats showed evidence of insight. Harlow (53) gave each of eight monkeys 112 two-choice discrimination learning problems; after 8 to 11 trials on each problem, the positive-negative values of the stimuli were reversed for the next eight trials. During the last 56 of the 112 problems, the average percentage of correct responses on the second trial of the reversal was 98, showing immediate learning. The progressive reduction of errors on successive reversals is termed a "discrimination reversal learning set." Harlow *et al.* (54, 56) give convincing evidence that monkeys manipulate the manipulatable without any need-reduction or reinforcement in the usual sense of these terms. Within limits, the rate of opening devices increased, presumably as a function of increasing skill with practice, and in this respect at least, the activity may be said to exhibit learning. The apparent absence of any effort by the monkeys to reassemble the device—repeatedly to "open" but never to "close" the devices—seems rather surprising and worthy of further study. Nissen (108) trained a chimpanzee in a set of discrimination problems which required simultaneous responsiveness to five independent aspects of the stimulus-objects. It would appear that man's superiority in such "conditional behavior" lies in his ability to reduce complexity through the use of symbolic formulae, rather than in the number of discrete elements that he can handle in the "if a, and if b, . . . then x" situation. In a preliminary report, Hayes & Hayes (58) describe a generalized imitation or matching technique—"do this," "give me one like this"—as used with their home-reared chimpanzee. The results achieved indicate that this approach will be extremely useful in bringing to expression the ultimate capacities of this ape.

Chemical Factors in Behavior

Diet.—The two-volume work of Keys *et al.* (69) can only be mentioned here as a source of much information pertaining to inanition in man. McCulloch (94) and Ellson *et al.* (37) report negative results in giving glutamic acid to mental defectives. The latter authors gave their subjects several tests and two test-batteries; the only significant group differences were in a test of co-ordination, in which the experimental group was better in time and worse in error scores. Some IQ increases up to 19 points were noted, but they occurred in the control as well as in the experimental group. The authors stress the importance of ignorance on the part of those administering the tests as to which subjects are getting the amino acid and which the placebo. Pilgrim *et al.* (113) found that addition of glutamic acid or methionine to a

basal diet containing 18 per cent protein did not alter the maze performance of an inbred strain of albino rats. Varying the total protein content of the diet between 12 and 30 per cent affected the growth rates but not maze behavior.

Maier & Longhurst (92) report that absence of lactose in the diet resulted in significant inferiority of performance on Maier's three-table reasoning problem in three of four groups of rats studied. The absence of lactose appears to be more injurious to females than to males. The authors point out that lactose is found only in milk, and milk "is the only substance developed in the course of evolution to serve as the exclusive food in the most critical period of postnatal mammalian life." They refer also to the reported correlation, among mammals, between milk sugar level and weight of the adult brain. Griffiths (50) found that when rats were allowed to select their own diet, susceptibility to audiogenic seizures was alleviated (reduced to zero after about eight days) in those animals that showed excessive intakes of thiamine hydrochloride.

Hormones and drugs.—The number of cornified genital papillae of the glans penis of castrated rats decreases to zero within a month after gonadectomy, according to Beach & Levinson (16). Adequate doses of testosterone propionate counteract the decrease. Sex activity, in terms of frequency of ejaculation, is rather closely related to the number of papillae present. Histological study indicates that the papillae serve as accessory sensory structures, friction causing them to stimulate underlying tactile nerve endings. The authors point out that such alteration of peripheral sensitivity is only one of several ways in which the androgen influences sexual behavior.

Birch & Clark (19) report that "administration of estrogens to female-castrate chimpanzees under conditions in which sex-skin engorgement is inhibited, results in a drop in dominance status." It previously had been shown that when engorgement is not inhibited (by progesterone), estrogen increased dominance status of the castrates. It would appear that the central and indirect peripheral effects of estrogen may have opposite effects on dominance in female chimpanzees. Müssbichler & Umrath (105) report that pigment changes in the tree frog are determined by two hormones: when epinephrine is "predominant," the frog becomes light colored or yellow; with an excess of intermedin, the animal turns dark green or black. There is evidence for some nervous control of pigment changes, but this does not appear to be mediated optically.

The webs constructed by a retiary spider can be altered from the normal in different ways by administration of several drugs, according to Peters *et al.* (112). Veronal (diethylbarbituric acid; barbital) inhibits construction altogether. Perivition (N,α-dimethylphenethylamine) results in webs that are smaller than usual, irregularity of the adhesive filaments, and omission of radial fibers. Caffeine administration eventuates in webs whose gross foundation structures are present but lacking in regularity of the detailed structures.

Peripheral Mechanisms of Stimulation and Response

Three species of planarians were studied by Viaud (147). Using orientation as an indicator response, he found that the dermal spectral sensitivity curve increases from red to violet, whereas that mediated by the eyes has a peak at 530 mμ. Speed of locomotion, the author reports, is the expression of antagonistic stimulatory and inhibitory actions of light. The former is proportional to the log of intensity, is inversely proportional to wave length, and is probably mediated dermally. The inhibitory action varies with intensity, is maximum at 530 mμ, but also functions in *Planaria*, with and without eyes, in violet light. Kühn (74) describes skin-color changes in *Sepia* and *Octopus* which are determined by black, yellow, and orange chromatophores and by reflector cells (iridocytes). On a red background, there is maximum extension of the orange chromatophores. The results of a training procedure, in which shock was associated with yellow or blue, are cited as further evidence of color vision.

The peach-leaf louse (*Myzodes persicae Sulz*) obtains nourishment by piercing leaves with its proboscis and sucking, according to Moericke (102). The insect is positive (i.e., feeds) in the chromatic range from 660 to 500 mμ, and is negative from 500 to 400 mμ. The dividing line is sharp, and the color sensitivity is remarkably independent of intensity and saturation. If the insect is first exposed to light in the "negative" range and is then placed on gray, it gives the feeding reaction; with previous exposure to white, the reaction on gray does not occur. It is shown that the sensitivity is optically, not dermally mediated. Ergene (38) found that yellow larvae of locusts when placed on a green background became green at their next moulting; green larvae on a yellow background became yellow. Larvae placed on a homochromic background never changed colors. When the eyes were painted over, color changes no longer occurred.

Electrical potentials from the eyes of various insects were recorded by Autrum (8). Compared to the vertebrate eye, the mosaic eye of insects has an inferior capacity for spatial resolution, which is in part compensated by greater capacity for temporal resolution. Autrum identifies two types of insect eye: (*a*) high sensitivity but slow adaptation and low fusion frequency and (*b*) lesser sensitivity but very fast adaptation and efficient temporal resolution. The first type is found mainly in nonflying insects; the second type, in fast flying insects. Birukow (20) found differences in the spectral sensitivity curve and in color vision among five species of *Urodela* and five of *Anura*. No correlations were found between either absolute or relative numbers of retinal elements (rods, cones, ganglion, and "central" cells) and ability to discriminate colors.

Several species of night-flying butterflies respond by flight or death-feigning to tones in the range of 15,000 to 175,000 c.p.s., according to Schaller & Timm (120). The lowest intensity thresholds were at 40,000 to 80,000 c.p.s. Extirpation experiments showed the *Tympanalorgan* to be the

peripheral receptor concerned. It was observed that this response is elicited by flying bats.

Wohlfahrt (154) trained minnows to approach when one tone was repeated, to stay away when two tones were sounded (the lower of these two being of the same pitch as the positive tone). Discriminations of intervals as small as the minor third were obtained but the discrimination broke down when the two negative tones were an octave apart. By the method of absolute discrimination (one positive, a different negative tone) the octave was easily discriminated. Using a modification of Wohlfahrt's relative discrimination technique (the single tone negative, the two positive tones alternating repeatedly, instead of shifting once from lower to higher), Dijkgraaf & Verheijen (34) found discriminations in the minnow of as little as quarter-tones (3 per cent difference in frequency) at 400 and 800 c.p.s. They found the upper limit of hearing at 5,000 c.p.s. Differential pitch discrimination was possible at 1,250 c.p.s., but not at 1,600 and 3,200 c.p.s. Extirpation of the Weber-organ had no marked effect on tone discrimination, and it is concluded that the lagena alone is as effective as the entire pars inferior. New apparatus and techniques for investigating the mechanism of statoreception in fish are described by von Holst (62). He reports that when the component effects of gravity are properly differentiated, "an unsurpassed simplicity and technical perfection of the static apparatus" is manifested.

Several studies indicate that eating and drinking may be quantitatively reduced, but are rarely abolished, by mechanical distension of the stomach or by supplying adequate nutrition or water through a gastric fistula. Measured in terms of immediate drinking, introducing water into the stomach through a fistula had little effect in the case of dog and rabbit; it had much more effect in the hamster, rat, and guinea pig (2). In another study, however, it was found that the amount of drinking by esophagostomized dogs is significantly reduced by an inflated balloon or an equal volume of water in the stomach (146). A summation of oral-pharyngeal and gastric stimuli in metering the intake is thus indicated. Dogs given one-third of their normal daily food-consumption through a gastric fistula did not show any decrease in amount eaten; when given up to 190 per cent through the fistula, they were still eating 21 per cent of their usual consumption after two weeks under this regime (129). The findings are, in general, consonant with the notion that endogenous or habitual activities are intrinsically motivated, but the fact of quantitative reduction indicates that the motivation is either secondary or is composed of summating factors.

The results of Miller *et al.* (101) with rats having lesions of the hypothalamus pose an interesting problem in psychosomatics: when food was freely available, the experimental animals ate more than the controls, but when any one of several obstacles or difficulties was put in the way of getting food, the consumption of the experimentals dropped below that of the controls. Lacy (75) reports that normal human subjects are more consistent and more reliably differentiated by the patterning of their somatic responses to stress than by any one of the variables or by average reactivity. This is the case

even when only functionally related (cardiovascular) responses are taken into consideration.

Central Mechanisms of Motor Function

The recent development of electronic stimulators by which the various parameters of stimulation can be independently varied has permitted experimentation which significantly modifies previous concepts of representation in the motor cortex. Liddell & Phillips (87), in search of the reason for the frequent onset of focal epilepsy in the thumb, mouth, or hallux, suggest that frequency of discharge, rather than strict locus of pathology, may be the important determinant of the site of origin of fits. They compare the map obtained with weak single "square-wave" stimuli to that delineated by repetitive stimulation in the cortex of the same baboon. The former yields extensive areas devoted to a few parts, whereas the latter shows narrow areas in which all parts of the body are represented, approximating the traditional arrangement of motor points. Thus, depending on parameters of the stimulus, there are many valid maps of the motor cortex, or in a strict sense, no valid map.

The question of whether "muscles" or "movements" are represented in the motor area continues to excite investigation. The protagonists of muscle-mosaic theories support their contention largely by the observation of responses of single muscles under conditions of threshold stimulation. Gellhorn (46) reports a study in which responses to threshold stimuli, applied to the precentral gyrus in monkeys, were measured by multiple electromyograms. Activity in functionally related muscle groups, instead of in single muscles or muscle fibers, was found. Such responses were obtained from all parts of the motor cortex, there being no detectable difference between areas 4 and 6 in this respect. In an experiment on the modification of motor response by concomitant proprioceptive impulses induced by fixation of joints at acute or obtuse angles (45), he found that such excitation not only influenced the behavior of the stretched muscle, but also the response of muscles functionally allied to the one from which the proprioceptive discharges were originating. Both of these studies lend support to theories of movement representation; the latter also points to a further source of variation in cortical map-making. Although experiments within the framework of this controversy have furnished important data, their meaning may have to be reinterpreted in the light of newer conceptions. Bonin (22) believes that the question of muscle- versus movement-representation loses much of its importance when the motor cortex is viewed as only one link in the chain of structures underlying action. The cortex can influence the ventral horn cells only indirectly through spinal internuncial pools, and its role may well be conceived as that of "setting the gain" of servomechanisms. The motor area itself receives afferent messages which must certainly be cast in terms of future positions or movements of joints.

The status of knowledge of cerebral motor function has been critically examined by Denny-Brown (33). From analysis of the disorders following

cortical lesions in relation to remaining motor behavior, he concludes that what is lost in cortical "paralyses" is a general facilitation of motor function affecting items integrated at all levels of the neuraxis, and not the power of contraction of any particular muscles nor even any specific movements or kind of movement. As he points out, this view is similar to that expressed by Lashley in 1924 (79). Although within area 4 there are "areas of relative concentration of some anatomic regions of musculature," Denny-Brown emphasizes that the sense in which these parts are represented is still unclear. Since finally, and relative to the strength of motivation provided by the situation, all movements of a part are possible despite removal of its cortical area, he suggests that the ablation experiment should be directed toward the unequivocal definition of the stimuli which elicit the behavior remaining and of those which are no longer adequate. His own experiments and clinical observations lead him to the conclusion that motor disorders, e.g., spasticity, are not "release" phenomena in the sense of loss of a supposed inhibitory center or pathway, but rather are the result of disequilibria between reflex-systems which, in the normal organism, are balanced and complementary. Study of the phenomena of grasping and placing indicate that the Rolandic cortex is particularly concerned with the modification of proprioceptive stretch reflexes by tactual components.

Welch & Penfield (151) report three cases of infantile hemiplegia in which atrophic sensorimotor cortex appeared to be augmenting the spinal stretch reflexes. Large removals in this zone resulted in decreased spasticity and improved motor performance, in spite of the fact that the cortex excised had been capable of producing movements in the paretic extremities when stimulated electrically. Among recently reported results of hemispherectomy in similar cases (73, 109), weakness and spasticity have often been ameliorated. The frequent therapeutic effect of hemispherectomy in both motor and mental spheres may be due to removal of abnormal influences on the function of the normal hemisphere, as reflected in changes of its electroencephalogram.

The traditional role of the pyramidal tract in voluntary movement has been questioned by Lassek (82), who attempted to correlate quantitative degeneration in this tract with degree of voluntary motor deficit in a large number of cases. Paralysis of voluntary movements can occur with total, severe, moderate, slight, or no degeneration in this tract. In view of these findings, Lassek suggests that if the pyramidal tract plays a major part in voluntary motor function, the possibilities of inhibition or lack of adequate excitation of its cells of origin must be considered. Lassek (83) also reports that total posterior rhizotomy (C2 to T4) in the monkey produces voluntary motor deficits which are more severe and enduring than those following unilateral removal of the motor cortex.

Nervous Structure and Sensory Processes

Cortical areas.—Further information on cerebral organization of sensory function has been obtained by the evoked potential technique. Hamuy

et al. (52) have mapped somatic areas I and II in the dog and find that the latter area has a well-organized somatotopic projection. Intracortical connections of somatic I and II in the cat have been found by Sencer (128), and Barnard & Woolsey (13) have shown that somatic I and II also are interconnected through the cerebellum. An approach to mapping the somatosensory receiving area in man by evoked potentials has been made by Woolsey & Erickson (155). Results so far appear to agree closely with those obtained by stimulation of the cortex and observation of the locus of referred sensation. Thompson *et al.* (140) have mapped visual areas I and II in the rabbit. Since in both cat and rabbit, visual area II (which has been homologized to area 18) is in contact with area I on only one border, they express doubt that area 18 completely surrounds the striate cortex in the monkey. Rosenzweig (116) reports a comparison of the electrical responses evoked at the auditory cortex by independent stimulation of the two ears with clicks. The boundaries of the auditory cortex are the same for both ears, but there are differences in wave form. The amplitude of the responses is larger and the threshold is lower in the contralateral hemisphere. A polysensory area, which is responsive to auditory, vestibular, proprioceptive, and tactile stimuli and has the characteristics of a primary projection area, has been located by Mickle & Ades (100) on the inferior part of the anterior ectosylvian and anterior suprasylvian gyri in the cat. The authors suggest that it may be the region in which integration of the stimuli necessary for the maintenance of spatial orientation takes place.

Central pathways.—The characteristics of central sensory paths have been studied by electrical and histological methods. In the cat (1), recording of the responses to acoustic clicks at successively higher levels of the projection path reveals progressive temporal dispersion of impulses and multiplication of responding elements to a single stimulus. In the olfactory system, on the other hand, counts of the receptors and of the mitral cells in the rabbit (4) indicate spatial summation at the level of the olfactory bulb of the order of a thousand receptors to one central relay. Adrian (3) emphasizes that the discharge of the mitral cells, whose fibers make up the olfactory tract, is influenced by the level of activity of the large number of accessory cells of the bulb as well as by impulses from the receptors. Accessory cells are not found at the comparable level of the visual pathway. This difference implies the possibility of "a much greater variety of response to an incoming message" in the olfactory than in the visual system. Lashley (80) has reported that in the rat nearly 300 retinal elements feed into each thalamocortical path. The numbers of discriminable intervals in the visual field and of cells in the lateral geniculate are about equal, which suggests that each acuity unit is represented by a single pathway. Between geniculate body and cortex, there is some dispersion. Another study (29) from Lashley's laboratory finds spread of impulses from thalamus to cortex in the monkey. In contrast to the ratio in the rat, it was estimated that there are about three times as many central pathways as there are discriminable intervals in the monkey's visual field. The authors suggest that this increase may be

related to trichromatic vision. A similar hypothesis has been advanced by Chang & Kaada (27), who have analyzed the primary cortical electrical response to optic nerve stimulation into six components, three of which are assignable to fibers from small, medium, and large geniculate body cells, respectively. They likewise suggest that the functional significance of these three separate thalamo-cortical routes may lie in color vision.

Thresholds.—Factors affecting sensory thresholds or their presumed correlates have been studied in a number of modalities. Thompson (139) reports an experiment on human subjects in which sensory nerves were stimulated electrically and resultant masking of sensations of touch and of pressure were measured. Enormous differences in threshold were found between areas of body surface supplied only by the stimulated nerve and areas supplied by other nerves as well. The implications of this finding for the relative arrangements of sensory centers and peripheral nerve-distributions are pointed out. Graham *et al.* (48), in an investigation of the neural mechanisms of itch, show that itch is masked within the area of hyperalgesia produced by faradic stimulation of a cutaneous nerve or by prick within the same dermatone. They suggest that itching may depend on heightened facilitatory activity in spinal internuncial circuits. King *et al.* (70) have studied cutaneous pain thresholds in the extremities of six patients before and after unilateral lobotomy performed for pain relief. Five patients experienced relief, and in four of these, pain thresholds were slightly lowered bilaterally. The one unrelieved patient had bilateral elevation of threshold. It would be desirable to know how long after operation these tests were made, in view of the finding of Chapman *et al.* (28) that decreased tolerance to thermal pain after bilateral lobotomy tends to be temporary. Decreased tolerance persisted, however, in the one patient of their series whose operation was done for relief of intractable pain. Keats *et al.* (68) present evidence that pathological as well as experimental pain can be reliably quantified in terms of its relief by measured doses of analgesics.

The effects of occipital and frontal lesions on thresholds of flicker fusion have been compared by Battersby (14) using human subjects. No depression of critical fusion frequency was found in the perimetrically normal fields of the cases with frontal injury, but in the perimetrically normal half fields of the occipital cases, there was a significant depression of the critical fusion frequency. Teuber & Bender (138) report that apparent movement is experienced across acquired scotomata, whether of retinal or of central origin; thresholds of such motion, although differing among the three groups studied (normal controls, cases of retinal scars, cases of occipital injury), are not significantly different within groups when movement is across a scotoma or across an intact region.

Discrimination.—Cortical function in audition has been investigated by Meyer & Woolsey (99). In order to produce lasting impairment of the cat's ability to discriminate small differences in frequency, it was found necessary to remove bilaterally all of auditory areas I and II, somatic area II, and the

posterior ectosylvian region. Animals who could not discriminate frequencies could nevertheless acquire simple conditioned responses and could discriminate small changes in intensity. Neff *et al.* (106) report that bilateral ablation of auditory areas I and II is followed by decreased ability to discriminate position of sound in space. An even more striking effect is the variability in performance level. In regard to the mechanism of localizing a source of sound, Rosenzweig (117) has pointed out that this ability requires a nervous mechanism which is sensitive to interaural time differences of only fractions of a millisecond. He reports that the relative amplitudes of responses in the two hemispheres are in fact altered by time differences of this order.

Habits of differential reaction to visual and to somesthetic stimuli in monkeys have been studied by Blum *et al.* (21) before and after bilateral ablation of three cortical regions: parietal lobe, temporal lobe, and parieto-temporo-preoccipital region. The first of these extirpations affected principally the somesthetic habits; the second and third affected the visual habits most consistently and severely, although in the third group there were also somesthetic deficits. The losses were in general recoverable, but more training was required postoperatively than in initial learning. Study of thalamic retrograde degeneration showed that such losses can occur without degeneration in the visual and somesthetic relay nuclei. Several animals with parieto-temporo-preoccipital ablations had subsequent bilateral prefrontal removals (30). Visual discrimination habits were retained, but there were recoverable losses in a visual conditional reaction and in some of the somesthetic tests. The data from these experiments were interpreted as indicating that, within the associative cortex, there are functionally specific discrete foci, supplemented by overlapping fringes of secondary significance.

Harlow (55) has compared the effects of somewhat similar cortical ablations in monkeys on visual discriminations, an oddity problem, and delayed reaction. After hemidecortication (sparing the motor and first somatic areas), half the subjects had a large lesion of the posterior associative area, and half a large lesion of the frontal associative area of the other hemisphere. The visual habits suffered most in the former group, and delayed reaction most in the latter; both groups were inferior to normals on the oddity problem. Harlow likewise suggests that the results indicate that the different functions tested have foci and gradients of representation in the cortex.

Other perceptual functions.—Since Bender and his co-workers revived interest in the method of double simultaneous stimulation, many cases have been reported in which defects were revealed by its use. The neural basis of the phenomenon of "extinction" and its relation to elevations of sensory thresholds, disturbances of local sign, disorders of the body scheme, and pathological failure of attention are still unsettled questions. Bender & Nathanson (18) report a case in which topographic patterns of allesthesia, extinction, and errors of localization were studied. These changes had somewhat similar distributions, although their extent differed. All were strictly limited, despite evidence of diffuse brain disease and general de-

mentia. Under the influence of amobarbital (5-ethyl-5-isoamylbarbituric acid; Amytal), the zones of allesthesia spread. Displacement of one stimulus with double stimulation was observable for a longer period than any of the other changes. Bender *et al.* (17) have also investigated errors of localization in normal adults, children, and patients with organic brain disease. In all, a consistent pattern of perception emerged on double stimulation. The face is most dominant (stimuli tend to be displaced to it) and the hand, least dominant. Thus, the prominence and consistency of displacement in cases of brain lesion appears to be an exaggeration of the normal pattern, especially that of children. Critchley (31) has reviewed the literature on the effects of double stimulation and argues for an interpretation of the resultant phenomena in terms of inattention, in spite of the fact that directed expectancy does not always modify them. The individual with central nervous injury cannot easily attend to competing stimuli, and the one which proceeds from a segment of higher limen, or from a region which has become less obtrusive in the body scheme, suffers. He suggests that the parietal lobe is a nervous mechanism of particular importance to local attention.

Other clinical studies relevant to cerebral organizations of complex perceptual functions include analyses of disorders of visual spatial perception (96) and of anosognosia (118), which, although uncommon, are when present frequently associated with lesions of the minor hemisphere. It is noteworthy, however, that no disturbance of spatial orientation or of the body image follows surgical removal of either hemisphere (73). The consistent forms assumed by spatial and temporal disorientation in organic brain involvement have been investigated (150, 153). A study of the age at which children are capable of having phantom limbs after amputations, and the delay in their appearance in some cases, is of particular interest (115).

Perceptual theory.—Köhler & Fishback (72) report that the destructive effect on the Müller-Lyer illusion of repeated trials is impeded by massed practice and is accelerated by rest periods. These results are interpreted as supporting the theory of satiation, and the possibility is suggested that memory traces may be weak patterns of satiation. The recording of direct currents from the heads of human subjects during auditory and visual stimulation has been continued, and experiments have been begun on the exposed cortex in animals (71).

Characteristics of the percept which indicate interaction of elements in the stimulus situation have played an important part in the development of "field" theory. The cortex is usually proposed as the site of this interaction. Two recent experiments suggest that brightness contrast (7) and many well-known illusions (104) are based in the retina, rather than at higher nervous levels. Another experimental criticism of field theory is offered by Lashley *et al.* (81), who attempted to produce distortions of the visual field in monkeys by shortcircuiting the presumed field currents. No disturbance of visual function was found. The failure of field theory to provide a mechanism for translation of the perceptual pattern to the efferent pattern is a fundamental weakness, the authors believe.

A possible clue to a mechanism by which cortical areas exert mutual influences is found in the experiments with photic flicker reviewed by Walter (148), who has found that activation of a sensory area at certain frequencies by physiological stimuli can induce frequencies in other regions which are multiples or submultiples of the original. The proportion of these induced frequencies appears to be associated with the psychological state of the subject.

Autonomic Components of Behavior

A brief summary in English of the experiments of W. R. Hess has been prepared by one of his co-workers (95); it describes the autonomic effects of stimulating diencephalic centers in unanesthetized cats. Such effects, which are isolated and difficult to interpret under anesthesia, were observed by Hess as meaningful elements in co-ordinated behavior of unrestrained animals. Stimulation of different regions may produce functionally dissimilar patterns of behavior containing similar specific end-organ responses, e.g., salivation may be produced as one element in both a thermoregulatory and a digestive complex; in the former case, it is associated with panting; in the latter, with licking and sniffing. Similarly, micturition may occur as an isolated response when the stimulus causes bladder emptying directly or may be associated with search for an appropriate site and assumption of normal posture. Hypothalamic stimulation produces direct visceral effects, whereas more anterior stimulation produces co-ordination of somatic and visceral activity. Hess has found a "hypnogenous" zone in the thalamus and subthalamus, stimulation of which produces "true sleep," a state in which the cat curls up in a corner but can be roused by noise or food odor. "Adynamy," or loss of initiative and interest, is the result of destruction of the "dynamogenous" zone. Hess believes that the diencephalon controls autonomic activity and that the cortex can exert only an indirect influence through the diencephalon.

The cortical influence on autonomic activity has been clarified by observation of the effects of stimulation and of surgical procedures in the cingulate, orbital, and temporal regions of man and animals. In unanesthetized cats, Hess *et al.* (61) have found that stimulation of the cingulate gyrus evokes contraversive movements and sympathetic effects. Ward & LeQuire (149) have elicited suppression of spontaneous walking from the same area. Glees *et al.* (47) find that restlessness, hyperactivity, and fearlessness follow cingular lesions in monkeys, but that these changes disappear within three months of operation. Marchi staining techniques show that the anterior cingular region projects mainly to the orbital surface, but not to the thalamus or to the brain stem reticular substance. Sloan & Jasper (132) report that stimulation of anterior limbic (cingular) and infralimbic areas produces general changes in spontaneous electrical activity of the cortex. These changes may be of various and even of opposite kinds, but they are similar in temporal and spatial characteristics, and all are mediated by cortical-subcortical mechanisms. It is suggested that the limbic and infra-

limbic areas have an intimate relation to the reticular formations of diencephalon and brain stem. This relation is inferred from the character of the results of cortical stimulation, and not from the demonstration of an anatomical pathway. The conception of these regions as regulatory in function explains why both facilitatory and inhibitory effects can be obtained from the same area under different conditions.

The close connection between the anterior cingulate and orbital regions has been mentioned. Babkin (9), reviewing his experiments on gastric motility, reports that inhibition occurs with stimulation of the cingulate region or of the orbital surface and that the mechanism is depression of hypothalamic parasympathetic centers. Stimulation of the orbital surface can also produce opposite effects on gastric motility. Ablation of the cingulate region increases motility, whereas orbital surface extirpation has no such effect. Various other autonomic responses can be elicited from the orbital region (119). These effects vary with frequency and form of the stimulus. Foci of maximal excitability for blood pressure and respiratory changes are found within area 13 or FF. Other experiments (141) suggest that such effects, obtained from either the orbital or cingular regions, may be mediated by the hypothalamus and brain stem reticular formation.

A study of the relation of the frontal lobes to autonomic reactivity and to emotion in dogs and cats (135) reveals a species difference. Progressively larger ablations from the frontal lobes produce an increasing "reaction of resistance" to restraint and autonomic hyperreactivity in dogs, whereas cats become more friendly and lose their usual reaction to noxious stimuli. Lichtenstein (86) reports loss of an acquired feeding inhibition and associated anxiety symptoms following lobotomy in dogs, although lobotomized dogs can acquire such inhibitions in normal time. In monkeys, transient tameness and hyperactivity are reported to follow lobotomy (41).

Smith (133) reports that loss of fear reactions, in this case associated with hypoactivity and oral compulsive behavior, results also from extirpation of the pyriform-amygdaloid-hippocampal complex in the monkey. Thomson & Walker (142) find that lesions in the amygdala, uncus, or hippocampus of monkeys produce tameness, fearlessness, and asocial behavior. Epileptogenic foci in the temporal lobe of man may cause attacks in which fear is a prominent symptom (110). Liberson *et al.* (85) report that stimulation of the uncus in conscious man produces profound changes in respiration and in state of consciousness; only slight changes follow stimulation of the cingular, orbital, and superior surface areas of the prefrontal region. Although olfactory function has traditionally been assigned to the complex of which the uncus is a part, these studies are in agreement with other recent evidence suggesting that its role is not solely, or perhaps not at all, in olfaction.

Cognition and Cerebral Interrelationships

Effects of brain damage.—Boycott & Young (23) report that, in the octopus, loss of a simple acquired sensory-motor habit (approach to a crab

when it is presented alone, and avoidance when a white plate accompanies it) follows removal of either of the two uppermost lobes of the brain. These lobes are associative in the sense of being jointly concerned with several senses. Removal of no other lobe produces the same results.

Two studies have been concerned with the effect of brain lesions on intelligence in the rat, as estimated by behavior in relatively free situations. Lansdell (78) has compared the effects of large unilateral, bilateral posterior, and bilateral anterior operations. On a modified Hebb-Williams test, the posterior lesion group was significantly worse than the unilateral, and the unilateral was significantly worse than the frontal, which did not differ from the normal controls. Van der Horst (63), who has evaluated the results of operation by the behavior of his rats on ladders, running boards, jumping platforms, etc., placed in their cages, also finds locus of lesion to be important, relative to the particular sensory data the problem requires.

Meyer *et al.* (98) tested one normal and two operated groups of monkeys on delayed response. Group *A*, with hemidecortication (sparing areas 4, 6, 3, 1, 2), was indistinguishable from the normals, but group *B*, with ablation of the lateral prefrontal region in addition to a similar contralateral hemidecortication, suffered almost complete loss, notwithstanding the fact that the medial and orbital surfaces of one frontal lobe were intact. An important difference between *A* and *B*, both of which were hyperactive, appeared to be that, as the food-well was being baited, those with the unilateral lesions would glance or pause, whereas those with the additional contralateral frontal excision would only accelerate their activity. The effects on delayed response of bilateral leucotomy or of unilateral destruction of the thalamic nucleus which projects to the prefrontal region have been reported by Freudenberg *et al.* (41). In the first few weeks after operation, the two leucotomized monkeys performed well on the problem, but one lost the ability after the thirty-fifth day. The thalamotomized animal could also do the problem until the twentieth postoperative day. Such findings emphasize the fact that loss of tissue is not the only cause of the sequelae of operation.

The nature of the change following leucotomy in human subjects has been studied by Landis *et al.* Analysis of the kind of errors made on the Porteus maze (76) indicates that increase in "confusion errors" (defined as entering a blind at choice points where the path is long or complex before the blockade is reached) is mainly responsible for the drop in score. Consideration of the answers on a homograph test (77), in which the patient has to give as many usages as possible to a list of words in a limited time, gives little support to the suggestion that postoperative improvement may result from increasing use of other methods of achieving a given result. Ten days after operation, there is some substitution of new usages for ones given preoperatively, but by 90 days afterward, most of the new usages have disappeared and most of the preoperative responses have reappeared. The authors advance the alternative hypotheses that either peripheral attention

or the "feeling of familiarity" is decreased in an unspecific way by leucotomy.

Data of possible significance for the neural basis of behavioral changes after leucotomy are provided by pneumoencephalographic studies (97) of the brains of a randomly selected group of patients before and up to 19 months after surgery. The most prevalent finding was of apparent progressive obliteration of the subarachnoid space. Such a condition would result in decreased absorption of the cerebrospinal fluid with consequent external hydrocephalus and was correlated with widespread degenerative changes found at necropsy. Although it is generally held that cutting of the thalamocortical radiations is the essential neural alteration in leucotomy, this evidence of diffuse cortical atrophy must also be taken into account. Another study (157) urges that the great mass of efferent projections which connect the frontal lobes to all levels of the neuraxis, and which are found to be degenerated after leucotomy, be considered in schemata of anatomicoclinical correlation.

Effects of convulsive shock.—A systematic exploration of the effects in the rat of a series of electroconvulsive shocks administered in rapid succession has been reported by Worchel & Gentry (156). The principal problems considered are those of the effects on new learning versus retention, retention of a barely learned versus an overlearned habit, and retention of an older versus a more recently acquired habit. It was found that six successive shocks impaired retention, but not new learning, of a complex maze habit and that overlearned and older habits were more resistant to shock effects than such habits when barely or recently learned. The authors postulate a "place" theory of learning, i.e., that shock selectively affects certain cerebral areas, which are essential to memory but not to new learning of a maze. With regard to the resistance of overlearned material, they theorize that shock affects the cortex rather than subcortical centers and that the more habitual a response becomes, the more it is delegated to subcortical parts. In the reviewers' opinion, the weight of existing evidence is contrary to both these interpretations.

Whether or not electroconvulsive shock has a selective effect on memories has been investigated in several studies. Braun & Patton (24) found that a series of such shocks, when applied to rats, disorganized recently acquired habits and reinstated those previously learned only when the recent habit was relatively more difficult. Simple habits tended not to be disrupted. Williams (152), using human patients, suggests that there are two sorts of memory on which shock has different effects. In this study, it was found that previous verbal responses to pictures tended to be repeated despite loss of recognition. Janis (66), also using human subjects, has presented indicative evidence that, in the case of personal memories, the original loss is indiscriminate, but that motivational factors affect what is later, with effort, recalled.

Integrative mechanisms.—Much of the evidence reviewed here tends to

support the hypothesis of "mass facilitation," expressed by Lashley (80). Briefly, the theory states that performance of any function depends upon two variables: (*a*) reduplicated neuronal patterns throughout an extensive functional system and (*b*) facilitation from any of a variety of sources. Habits and skills are not destroyed by any cortical ablation, provided some part of the functional system is preserved. Lesions of associative areas may produce amnesias or difficulties in learning through " . . . a lowered level of vigilance, a greater difficulty in activating the organized patterns of traces, or a disturbance of some broader system of organized functions." It is interesting to compare Lashley's theory with that of Penfield & Rasmussen (111), outlined in a monograph reviewing their observations of many years on the results of cortical stimulation and patterns of epileptic discharge. Their schema envisages a cortical locus for the memory trace with its center in the temporal lobe. They divide cortical areas into four groups: sensory and motor, elaboration areas representing acquired skills, areas especially concerned with perception and memory, and areas concerned with the elaboration of thought processes. The fourth group is located in the anterior frontal cortex and has a close connection with "the highest level of neuronal representation and re-representation" which is not in the cortex, but in the diencephalon. These divergent views of the mechanisms of association and memory may be partly a function of experimental material (animals versus epileptics), each of which has its limitations. Certainly in both man and animals, the dispensability of large areas of cortex requires theoretical rationale, as does the significance for normal functional organization of the patterns activated by pathological or artificial means.

Research disclosing elaborate systems of intracortical connections has continued, the most notable recent contribution being a comprehensive review of the results of local strychninization of the cortex of chimpanzee (12). Since its inception, the method of "physiological neuronography" has been one of the most important sources of data on cerebral interrelationships. Lately, however, failure to activate certain known nervous pathways by this method has been reported (40). Moreover, the capacity for causing a slowly spreading reduction in electrical activity, ascribed uniquely to the "suppressor bands," has been shown to be a general cortical property (131) probably related to impairment of pia-arachnoid function (93).

Integrative action has been ascribed to the diffuse projection system from the thalamic reticular formation (67). Appropriate stimulation of this system is capable of setting up in the cortex strains of rhythmic waves with the same frequency and form as spontaneous cortical activity. Contrary to what has previously been believed, lesions of the primary relay nuclei do not abolish spontaneous activity of the related sensory areas, nor does such damage prevent control of the area by the reticular system. It is hypothesized that the reticular system is a central co-ordinating mechanism, controlling the local and general background of cortical activity upon which afferent impulses act. Support to the suggestion of this integrative action is

given by the fact that the diffuse system projects mainly to the associative areas, avoiding the primary sensory areas (136). The alternative interhemispheric connections provided by this system may explain negative results obtained from section of the neocortical commissures (103) as well as from interruption of intrahemispheric paths (44). The responses whose spread has been studied so far are mainly electrical ones; the determination of the role of these connections in mediating sensory-motor integrations is a task for the future.

The question arises: Could this system correspond to Penfield's diencephalic center of consciousness? From existing evidence, the answer might be partially affirmative. The data on which the hypothesized center is based derive largely from observations of petit mal epilepsy. When the reticular response is linked with that of another system, stimulation of which causes 3-per-sec. waves, the 3-per-sec. wave and spike complex, characteristic of petit mal, result (67).

What of other possibilities of modulation and elaboration? Chang's experimental demonstration of cortico-thalamo-cortical reverberating circuits (26), which are independent of the thalamic system controlling spontaneous cortical rhythmicity, furnishes further validation of the frequent use of the concept of reverberatory activity in current theory. Another structure whose function may have been conceived too narrowly is the cerebellum, which enters into reciprocal circuits with cortex, diencephalon, midbrain, and thalamus (134). Evidence is cited that its role extends beyond that of co-ordinating muscular action. The suggestion is made that it potentiates or damps the activity of sensory and other centers as well.

LITERATURE CITED

1. Ades, H. W., and Brookhart, J. M., *J. Neurophysiol.*, **13,** 189–205 (1950)
2. Adolph, E. F., *Am. J. Physiol.*, **161,** 374–86 (1950)
3. Adrian, E. D., in *Physiological Mechanisms in Animal Behaviour*, 85–91 (Soc. Exptl. Biol., Cambridge Univ. Press, London, 482 pp., 1950)
4. Allison, A. C., and Warwick, R. T. T., *Brain*, **72,** 186–97 (1949)
5. Armstrong, E. A., in *Physiological Mechanisms in Animal Behaviour*, 361–84 (Soc. Exptl. Biol., Cambridge Univ. Press, London, 482 pp., 1950)
6. Aronson, L. R., *Am. Museum Novitates*, No. 1484, 1–26 (1951)
7. Asher, H., *Brit. J. Psychol.*, **40,** 187–94 (1950)
8. Autrum, H., *Z. Vergleich. Physiol.*, **32,** 176–277 (1950)
9. Babkin, B. P., *Gastroenterology*, **14,** 479–84 (1950)
10. Baerends, G. P., in *Physiological Mechanisms in Animal Behaviour*, 337–60 (Soc. Exptl. Biol., Cambridge Univ. Press, London, 482 pp., 1950)
11. Baerends, G. P., and Baerends-van Roon, J. M., *Behaviour*, Suppl. 1, 243 pp. (1950)
12. Bailey, P., Bonin, G. von, and McCulloch, W. S., *The Isocortex of the Chimpanzee* (Univ. Illinois Press, Urbana, Ill., 440 pp., 1950)
13. Barnard, J. W., and Woolsey, C. N., *Arch. Neurol. Psychiat.*, **65,** 238 (1951)
14. Battersby, W. S., *Am. Psychologist*, **5,** 271 (1950)
15. Beach, F. A., *Am. Psychologist*, **5,** 115–24 (1950)

16. Beach, F. A., and Levinson, G., *J. Exptl. Zool.*, **114**, 159–71 (1950)
17. Bender, M. B., Fink, M., and Green, M., *Trans. Am. Neurol. Assoc.*, **75**, 250–52 (1950)
18. Bender, M. B., and Nathanson, M., *Arch. Neurol. Psychiat.*, **64**, 501–15 (1950)
19. Birch, H. G., and Clark, G., *J. Comp. Physiol. Psychol.*, **43**, 181–93 (1950)
20. Birukow, G., *Z. Vergleich. Physiol.*, **32**, 348–82 (1950)
21. Blum, J. S., Chow, K. L., and Pribram, K. H., *J. Comp. Neurol.*, **93**, 53–100 (1950)
22. Bonin, G. von, *Essay on the Cerebral Cortex* (Charles C Thomas, Publisher, Springfield, Ill., 150 pp., 1950)
23. Boycott, B. B., and Young, J. Z., in *Physiological Mechanisms in Animal Behaviour*, 432–53 (Soc. Exptl. Biol., Cambridge Univ. Press, London, 482 pp., 1950)
24. Braun, H. W., and Patton, R. A., *J. Comp. Physiol. Psychol.*, **43**, 252–63 (1950)
25. Brown, W. L., and Gentry, G. V., *J. Comp. Physiol. Psychol.*, **43**, 226–30 (1950)
26. Chang, H.-T., *J. Neurophysiol.*, **13**, 235–57 (1950)
27. Chang, H.-T., and Kaada, B., *J. Neurophysiol.*, **13**, 305–18 (1950)
28. Chapman, W. P., Rose, A. S., and Solomon, H. C., *Am. J. Psychiat.*, **107**, 221–24 (1950)
29. Chow, K. L., Blum, J. S., and Blum, R. A., *J. Comp. Neurol.*, **92**, 227–39 (1950)
30. Chow, K. L., Blum, J. S., and Blum, R. A., *J. Neurophysiol.*, **14**, 59–71 (1951)
31. Critchley, M., *Brain*, **72**, 538–61 (1949)
32. Daanje, A., *Behaviour*, **3**, 48–98 (1950)
33. Denny-Brown, D., *J. Nervous Mental Diseases*, **112**, 1–45 (1950)
34. Dijkgraaf, S., and Verheijen, F. J., *Z. Vergleich. Physiol.*, **32**, 248–56 (1950)
35. Eibl-Eibesfeldt, I., *Z. Tierpsychol.*, **7**, 327–55 (1950)
36. Eibl-Eibesfeldt, I., *Z. Tierpsychol.*, **7**, 558–87 (1950)
37. Ellson, D. G., Fuller, P. R., and Urmston, R., *Science*, **112**, 248–50 (1950)
38. Ergene, S., *Z. Vergleich. Physiol.*, **32**, 530–51 (1950)
39. Ford, C. S., and Beach, F. A., *Patterns of Sexual Behavior* (Harper & Bros., New York, 307 pp., 1951)
40. Frankenhaeuser, B., *J. Neurophysiol.*, **14**, 73–79 (1951)
41. Freudenberg, R. K., Glees, P., Obrador, S., Foss, B., and Williams, M., *J. Mental Sci.*, **96**, 143–56 (1950)
42. Frisch, K. von, *Bees: Their Vision, Chemical Senses and Language* (Cornell Univ. Press, Ithaca, N. Y., 119 pp., 1950)
43. Frisch, K. von, *Experientia*, **6**, 210–21 (1950)
44. Gastaut, H., and Hunter, J., *EEG Clin. Neurophysiol.*, **2**, 263–87 (1950)
45. Gellhorn, E., *Brain*, **72**, 35–62 (1949)
46. Gellhorn, E., *Brain*, **73**, 267–74 (1950)
47. Glees, P., Cole, J., Whitty, C. W. M., and Cairns, H., *J. Neurol. Neurosurg. Psychiat.*, **13**, 178–90 (1950)
48. Graham, D. T., Goodell, H., and Wolff, H. G., *J. Nervous Mental Diseases*, **112**, 546–49 (1950)
49. Grassé, P.-P., and Noirot, C., *Behaviour*, **3**, 146–66 (1951)
50. Griffiths, W. J., Jr., *Science*, **112**, 786–87 (1950)
51. Guhl, A. M., *Physiol. Zoöl.*, **23**, 361–66 (1950)
52. Hamuy, T. P., Bromiley, R. B., and Woolsey, C. N., *Am. J. Physiol.*, **163**, 719–20 (1950)
53. Harlow, H. F., *J. Comp. Physiol. Psychol.*, **43**, 231–39 (1950)

54. Harlow, H. F., *J. Comp. Physiol. Psychol.*, **43**, 289–94 (1950)
55. Harlow, H. F., *Science*, **112**, 428 (1950)
56. Harlow, H. F., Harlow, M. K., and Meyer, D. R., *J. Exptl. Psychol.*, **40**, 228–34 (1950)
57. Hartley, P. H. T., in *Physiological Mechanisms in Animal Behaviour*, 313–36 (Soc Exptl. Biol., Cambridge Univ. Press, London, 482 pp., 1950)
58. Hayes, K. J., and Hayes, C., *Proc. Am. Phil. Soc.*, **95**, 105–9 (1951)
59. Hein, G., *Experientia*, **6**, 142–44 (1950)
60. Hess, E. H., *J. Comp. Physiol. Psychol.*, **43**, 112–22 (1950)
61. Hess, W. R., McDonald, D. A., and Livingston, R. B., *Federation Proc.*, **10**, 63 (1951)
62. Holst, E. von, *Z. Vergleich. Physiol.*, **32**, 60–120 (1950)
63. Horst, L. van der, *Acta Psychol.*, **7**, 247–55 (1950)
64. Hudson, B. B., *Genetic Psychol. Monographs*, **41**, 99–145 (1950)
65. Jacobs, W., *Z. Tierpsychol.*, **7**, 169–216 (1950)
66. Janis, I. L., *J. Nervous Mental Diseases*, **111**, 359–82 (1950)
67. Jasper, H., *EEG Clin. Neurophysiol.*, **1**, 405–20 (1949)
68. Keats, A. S., Beecher, H. K., and Mosteller, F. C., *J. Applied Physiol.*, **3**, 35–44 (1950)
69. Keys, A., Brožek, J., Henschel, A., Mickelsen, O., and Taylor, H. L., *The Biology of Human Starvation* (Univ. Minnesota Press, Minneapolis, 2 Vols., 1385 pp., 1950)
70. King, H. E., Clausen, J., and Scarff, J. E., *J. Nervous Mental Diseases*, **112**, 93–96 (1950)
71. Köhler, W., *Science*, **113**, 478 (1951)
72. Köhler, W., and Fishback, J., *J. Exptl. Psychol.*, **40**, 398–410 (1950)
73. Krynauw, R. A., *J. Neurol. Neurosurg. Psychiat.*, **13**, 243–67 (1950)
74. Kühn, A., *Z. Vergleich. Physiol.*, **32**, 572–98 (1950)
75. Lacey, J. I., *J. Comp. Physiol. Psychol.*, **43**, 338–50 (1950)
76. Landis, C., and Erlick, D., *Am. J. Psychol.*, **63**, 557–66 (1950)
77. Landis, C., Zubin, J., and Mettler, F. A., *J. Psychol.*, **30**, 123–38 (1950)
78. Lansdell, H. C., *Am. Psychologist*, **5**, 255–56 (1950)
79. Lashley, K. S., *Arch. Neurol. Psychiat.*, **12**, 249–76 (1924)
80. Lashley, K. S., in *Physiological Mechanisms in Animal Behaviour*, 454–82 (Soc. Exptl. Biol., Cambridge Univ. Press, London, 482 pp., 1950)
81. Lashley, K. S., Chow, K. L., and Semmes, J., *Psychol. Rev.*, **58**, 123–36 (1951)
82. Lassek, A. M., *Brain*, **73**, 95–102 (1950)
83. Lassek, A. M., *Anat. Record*, **109**, 376 (1951)
84. Levy, D. M., *Am. J. Orthopsychiat.*, **20**, 335–62 (1950)
85. Liberson, W. T., Scoville, W. B., and Dunsmore, R. H., *EEG Clin. Neurophysiol.*, **3**, 1–8 (1951)
86. Lichtenstein, P. E., *J. Comp. Physiol. Psychol.*, **43**, 419–27 (1950)
87. Liddell, E. G. T., and Phillips, C. G., *Brain*, **73**, 125–40 (1950)
88. Lindemann, W., *Z. Tierpsychol.*, **7**, 217–40 (1950)
89. Löhrl, H., *Z. Tierpsychol.*, **7**, 130–33 (1950)
90. Lorenz, K. Z., *Studium Generale*, **3**, 455–99 (1950)
91. Lorenz, K. Z., *Physiological Mechanisms in Animal Behaviour*, 221–68 (Soc. Exptl. Biol., Cambridge Univ. Press, London, 482 pp., 1950)
92. Maier, N. R. F., and Longhurst, J. U., *J. Comp. Physiol. Psychol.*, **43**, 375–88 (1950)

93. Marshall, W. H., *EEG Clin. Neurophysiol.*, **2,** 177–85 (1950)
94. McCulloch, T. L., *Am. J. Mental Deficiency*, **55,** 117–22 (1950)
95. McDonald, D. A., *Lancet*, **I,** 627–29 (1951)
96. McFie, J., Piercy, M. F., and Zangwill, O. L., *Brain*, **73,** 167–90 (1950)
97. Meschan, I., and Scruggs, J. B., *Arch. Neurol. Psychiat.*, **65,** 60–71 (1951)
98. Meyer, D. R., Harlow, H. F., and Settlage, P. H., *J. Comp. Physiol. Psychol.*, **44,** 17–25 (1951)
99. Meyer, D. R., and Woolsey, C. N., *Am. J. Physiol.*, **163,** 735 (1950)
100. Mickle, W. A., and Ades, H. W., *Federation Proc.*, **10,** 92 (1951)
101. Miller, N. E., Bailey, C. J., and Stevenson, J. A. F., *Science*, **112,** 256–59 (1950)
102. Moericke, V., *Z. Tierpsychol.*, **7,** 265–74 (1950)
103. Morin, F., and Goldring, S., *J. Comp. Neurol.*, **93,** 229–39 (1950)
104. Motokawa, K., *J. Neurophysiol.*, **13,** 413–26 (1950)
105. Müssbichler, A., and Umrath, K., *Z. Vergleich. Physiol.*, **32,** 311–18 (1950)
106. Neff, W. D., Arnott, G. P., and Fisher, J. D., *Am. J. Physiol.*, **163,** 738 (1950)
107. Nissen, H. W., in *Handbook of Experimental Psychology*, 347–86 (Stevens, S. S., Ed., John Wiley & Sons, Inc., New York, 1436 pp., 1951)
108. Nissen, H. W., *J. Comp. Physiol. Psychol.*, **44,** 9–16 (1951)
109. Obrador, S., and Larramendi, M. H., *EEG Clin. Neurophysiol.*, **2,** 143–46 (1950)
110. Penfield, W., and Flanigin, H., *Arch. Neurol. Psychiat.*, **64,** 491–500 (1950)
111. Penfield, W., and Rasmussen, T., *The Cerebral Cortex of Man* (Macmillan Co., New York, 249 pp., 1950)
112. Peters, H. M., Witt, P. N., and Wolff, D., *Z. Vergleich. Physiol.*, **32,** 29–45 (1950)
113. Pilgrim, F. J., Zabarenko, L. M., and Patton, R. A., *J. Comp. Physiol. Psychol.*, **44,** 26–36 (1951)
114. Räber, H., *Behaviour*, **2,** 1–95 (1949)
115. Riese, W., and Bruck, G., *Rev. neurol.*, **83,** 1–2 (1950)
116. Rosenzweig, M. R., *Am. Psychologist*, **5,** 270 (1950)
117. Rosenzweig, M. R., *Federation Proc.*, **10,** 112 (1951)
118. Roth, M., *Brain*, **72,** 89–111 (1949)
119. Sachs, E., Jr., Brendler, S. J., and Fulton, J. F., *Brain*, **72,** 227–40 (1949)
120. Schaller, F., and Timm, C., *Z. Vergleich. Physiol.*, **32,** 468–81 (1950)
121. Schiller, P. H., *J. Exptl. Psychol.*, **40,** 217–27 (1950)
122. Schneider, K. M., *Z. Tierpsychol.*, **7,** 485–558 (1950)
123. Scott, J. P., and Marston, M. V., *J. Genetic Psychol.*, **77,** 25–60 (1950)
124. Scott, J. P., and Marston, M. V., *Behaviour*, **2,** 135–43 (1950)
125. Seitz, A., *Z. Tierpsychol.*, **7,** 1–46 (1950)
126. Seitz, A., *Z. Tierpsychol.*, **7,** 275–86 (1950)
127. Seitz, A., *Z. Tierpsychol.*, **7,** 402–17 (1950)
128. Sencer, W., *Am. J. Physiol.*, **163,** 749 (1950)
129. Share, I., and Grossman, M. I., *Am. J. Physiol.*, **163,** 749–50 (1950)
130. Skard, Ø., *Acta Psychol.*, **7,** 89–109 (1950)
131. Sloan, N., and Jasper, H., *EEG Clin. Neurophysiol.*, **2,** 59–78 (1950)
132. Sloan, N., and Jasper, H., *EEG Clin. Neurophysiol.*, **2,** 317–27 (1950)
133. Smith, W. K., *Federation Proc.*, **9,** 118 (1950)
134. Snider, R. S., *Arch Neurol. Psychiat.*, **64,** 196–219 (1950)
135. Speakman, T. J., and Babkin, B. P., *Arch. Neurol. Psychiat.*, **63,** 433–43 (1950)
136. Starzl, T. E., and Magoun, H. W., *J. Neurophysiol.*, **14,** 133–46 (1951)
137. Steiniger, F., *Z. Tierpsychol.*, **7,** 356–79 (1950)

138. Teuber, H., and Bender, M. B., *Am. Psychologist*, **5**, 271 (1950)
139. Thompson, I. M., *Trans. Roy. Soc. Canada*, **42**, 165–74 (1948)
140. Thompson, J. M., Woolsey, C. N., and Talbot, S. A., *J. Neurophysiol.*, **13**, 277–88 (1950)
141. Thompson, W. C., and Bach, L. M. N., *J. Neurophysiol.*, **13**, 455–64 (1950)
142. Thomson, A. F., and Walker, A. E., *Arch. Neurol. Psychiat.*, **65**, 251–52 (1951)
143. Thorpe, W. H., in *Physiological Mechanisms in Animal Behaviour*, 387–408 (Soc. Exptl. Biol., Cambridge Univ. Press, London, 482 pp., 1950)
144. Tinbergen, N., in *Physiological Mechanisms in Animal Behaviour*, 305–12 (Soc. Exptl. Biol., Cambridge Univ. Press, London, 482 pp., 1950)
145. Tinbergen, N., and Perdeck, A. C., *Behaviour*, **3**, 1–39 (1950)
145a. Tinbergen, N., *Z. Tierpsychol.*, **4**, 1–40 (1940)
146. Towbin, E. J., *Am. J. Physiol.*, **159**, 533–41 (1949)
147. Viaud, G., *Behaviour*, **2**, 163–216 (1950)
148. Walter, W. G., *J. Mental Sci.*, **96**, 1–31 (1950)
149. Ward, J. W., and LeQuire, V., *Anat. Record*, **106**, 256–57 (1950)
150. Weinstein, E. A., and Kahn, R. L., *Arch. Neurol. Psychiat.*, **65**, 533–34 (1951)
151. Welch, K., and Penfield, W., *J. Neurosurg.*, **7**, 414–20 (1950)
152. Williams, M., *J. Neurol. Neurosurg. Psychiat.*, **13**, 314–19 (1950)
153. Williams, M., and Zangwill, O. L., *J. Mental Sci.*, **96**, 484–93 (1950)
154. Wohlfahrt, T. A., *Z. Vergleich. Physiol.*, **32**, 151–75 (1950)
155. Woolsey, C. N., and Erickson, T. C., *Trans. Am. Neurol. Assoc.*, **75**, 50–52 (1950)
156. Worchel, P., and Gentry, G., *Comp. Psychol. Monographs*, **20**, 95–119 (1950)
157. Yakovlev, P. I., Hamlin, H., and Sweet, W. H., *J. Neuropath. Exptl. Neurol.*, **9**, 250–85 (1950)

ABNORMALITIES OF BEHAVIOR[1]

By Joseph Zubin

Psychiatric Institute, Columbia University, New York, N. Y.

The two previous reviews of abnormalities of behavior were organized in strikingly different ways. Cameron (11) organized the data under the following topics: etiology; learning; induced behavior patterns; stress; frustration; anxiety; language and thinking; and fantasy, hallucination, and delusion. Taub (59), on the other hand, after introducing the chapter by a review of general progress in psychodynamics, followed the more conservative pattern of subdividing the field into broad diagnostic categories: psychosomatics; conduct disorders; psychotic disorders; psychoneuroses; mental defect and epilepsy; and organic brain disorders. These two reviews, covering the same field from two angles, have made it unnecessary for the present reviewer to make the exhaustive coverage of the field that his predecessors made, since progress in abnormal psychology is not made equally rapidly in all directions. The present review will, therefore, deal with the four outstanding currents in the field of abnormality of behavior of the last year (Janurary, 1950 to April, 1951). These are: (*a*) behavioral accompaniments of therapeutic and experimental alterations in brain structure and function (psychosurgery for relief of psychosis and intractable pain), (*b*) behavioral accompaniments of alterations in the internal environment (shock therapy, avitaminosis and stress—producing chemical agents, and starvation), (*c*) behavioral accompaniments of life stress, and (*d*) behavioral accompaniments of genetically based disorders. Furthermore, the review will limit itself as far as possible to quantitative studies in this field, since, in the last analysis, it is on the quantitative side that the psychologist's contribution chiefly lies. Special stress will be laid on the design, execution, and evaluation of the experimental and therapeutic procedures involved in the investigations to be reported.

Alteration of Brain Structure and Function

The tremendous impact that psychosurgery has made on current investigations is evidenced by the following books and monographs which were published between 1949 and 1951: Mettler (40, 41), Lewis (35), Freeman & Watts (15), Partridge (45), *Anatomo-Physiologie cérébrale á la Lumière des Lobotomies et Topectomies*, 1950 (12), Hebb (24), Fulton (16), Greenblatt *et al.* (18), Penfield & Rasmussen (46), Richter (53), Halstead (21, 22), Porteus (51), the report on the first International Conference on psychosurgery in Lisbon (52), the Anglo-American Symposium on Psychosurgery (4), the British Medical Bulletin (29) and the symposium on the Frontal Lobes (7).

[1] The survey of the literature to which this review pertains was completed in April, 1951.

It seems that just as the shock therapies served as a shot-in-the-arm for psychiatry in the 1930's, so has psychosurgery served the same purpose in the 1940's. The general aim of psychosurgery is to bring about such alteration in the behavior of the mental patient that he will be freed of the need for hospitalization. The mechanism whereby such alteration in behavior is effected remains a mystery despite much theorizing on the subject. In the process of bringing about such alterations, two types of questions arise. One deals primarily with the practical psychiatric problems involved, the other with the more scientific aspects. On the practical side arise such questions as: what type of patient is suitable for psychosurgery; what type of operation is most effective, what role do "total push" and psychotherapy play in the improvement; what is the role of the family situation in the rehabilitation process; and which patients can be benefited by operation even though they do not improve sufficiently to return home? On the scientific side, such questions arise as: what psychological functions are mediated by the frontal lobes; why do some patients who receive the same type of operation show no similarity in behavioral changes; what is the role of quantity of tissue removed or severed in contrast to the location of this tissue; why does the psychosis persist in some patients, temporarily diminish, and then return in others, and completely disappear in still others?

The mechanism whereby the operation brings about improvement is still unknown. Theories range from the postulation of a diaschisis factor to the postulation of a specific interference with fibers possiblv mediating anxiety (4). There is at least one long range follow-up study which seems to indicate that the operation itself is no more effective than mere passage of time or "total push" in bringing about improvement, and that the basic factors to be considered are type and stage of the disease process in which the patient finds himself at the time of therapy (68). Furthermore, in this study, prognosis based on psychological test performance was successful in predicting outcome regardless of operation.

More light was cast on the importance of the frontal lobe in psychological function than on the mechanism of improvement from psychosis in the recent Columbia-Greystone I Study (40). However, much of the light cast was of a negative type, that is, it served to "debunk" present unfounded opinion or prejudiced belief rather than to develop new insights. Contrary to generally accepted dogma, the frontal lobes can be interfered with in diverse ways or even removed without producing detectable deleterious effects, provided the agranular tissue is left intact. Thus the general tendency of the last 50 years for neurosurgeons and neurologists to regard the frontal lobes as a "catch-all," where any function not localized elsewhere can find a peg to hang on, has come to an end.

The only psychological characteristic that seems definitely interfered with after psychosurgery is level of anxiety, which is considerably reduced in a qualitative rather than a quantitative way. The patients still report the

presence of anxiety, hallucinations, or pain, but these conditions are no longer intractable. Certain temporary changes occur which disappear within the first several weeks or months. By the end of the third month, most of the psychological functions have returned to their preoperative level, or have even exceeded it. Other psychological deficits, some of an apparently permanent type, were noted in patients in whom the agranular portions of the frontal lobes were invaded (Brodmann's areas 4, 6, 8). A general lag in the ability to benefit from the practice afforded by the repeated application of the tests was noted in the operated patients. Whether this lag represents a basic interference with function or whether it reflects the poorer quality of registration after operation is still an open question.

In contrast with these specific findings, there are several reports which yield a somewhat different picture, although in the main there is more agreement than disagreement. There is general agreement that the type of operation is not as important in determining the outcome as was formerly believed, provided no incidental damage results. Thermocoagulation—destruction of selected portions of the frontal lobe by means of a heated template (41), venous ligation—shutting off the venous drainage of the external surface so as to force drainage through the alternate venous system (41); thalamotomy —surgical undercutting or destruction of the dorsal medial nuclei of the thalamus, transorbital lobotomy (15) and classical types of lobotomy, topectomy, and selective cortical undercutting (4) yield equivalent results in most of the reported studies. It should be noted that until now most of the psychosurgical operations have depended upon some type of cytoarchitectonic map. The theories of brain function emanating from response to electrical or strychnine stimulation have not yet been introduced into psychosurgical design (9a). On the basis of cytoarchitectonics alone, two divisions of the frontal cortex exist—granular and agranular. It has already been pointed out that the latter when injured produces definite deficiencies in intellect and behavior. Otherwise no localization has thus far been demonstrated for any function.

Regarding the influence of quantity of tissue removed or interfered with, less agreement exists. The Columbia-Greystone I Study (40) found little if any relationship between amount of tissue removed and psychological change or improvement. Meyer [see (53)], on the other hand, is of the opinion that there is a quantitative relation between improvement and brain injury, within certain limits.

There is general agreement that not all patients, but at least one third, seem to benefit from psychosurgery. In the case of severe neurotics (41), however, the proportion of improvement is very high.

Considerable agreement exists regarding certain transient changes occurring immediately after psychosurgery and disappearing within three months. These changes are not consistent from patient to patient, but the presence of some alteration of function is undeniable. Although there are

various ways of explaining them, it has been suggested that the hypotheses might well fall under the triad: vigilance, zeal, and anguish (34). Vigilance is the concept propounded by Head (23) and its lack would be exhibited in such behavior as sleepiness, inattentiveness, and unawareness. Zeal refers to the motivational system. The interest and enthusiasm of the patients is less readily evoked in the immediate postoperative period than in the preoperative, and follow-up, postoperative periods. Zeal seems to return more slowly than vigilance, and sometimes remains at a lower level than in the premorbid state. Greenblatt *et al.* (18) found that 60 per cent of their patients showed lessened initiative. Anguish reduction, however, is the one change that seems to persist, at least in patients who are regarded as improved. Whether this change in anguish is referable to the operation, or is an indirect result of the lifting of the psychosis, which in turn may or may not be related to the operation, is still a moot question.

The other changes that have been alleged to result from psychosurgery have not been established well enough to merit general acceptance. The reduction in intelligence which has been claimed by several workers [Penfield & Rasmussen (46), Partridge (45), Greenblatt (18) and Porteus (51)] is not borne out by most investigations. Partridge, who has written the most readable personal account of the follow-up of some 300 cases, seems to have concluded, despite his own evidence to the contrary, that an intellectual deficit is created in nearly all cases. The heritage of the last generation in vague reasoning regarding the associative functions of the frontal cortex is reaping a harvest of persistent prejudice even in our keenest investigators and authors. Freeman (4), too, insisted that all operated cases show some deficit intellectually, even though tests on his own patients did not demonstrate it. When confronted with the absence of evidence for intellectual deterioration, the proponents of such hypotheses retort that the present day tests are not sensitive enough to detect the defects. It is urged that creative intelligence, if not "psychometric" intelligence, is affected. Several studies on this question have been undertaken. Ashby & Bassett (6) on the basis of quantitative tests found no evidence for loss of creativity, and the present author in a similar study of the Columbia-Greystone patients (67), also failed to find any evidence for loss of creativity.

Another indication of the tenacity with which psychologists adhere to the belief that the frontal lobes are important in psychological functioning is demonstrated in the study of Robinson (54). After repeating the claims that the abstract-concrete attitude is affected by psychosurgery and that planning ability and maintenance of set and capacity for prolonged attention are affected adversely, she proceeded to develop two new tests—The Self-regarding Span Test and the Sensibility Questionnaire. The former measures preoccupation with the self, and the latter measures the degree of one's concern with the future and with the opinions held by other people regarding one's own behavior. In all three of these functions, lobotomized

patients were found to be wanting. The design of Robinson's experiment leaves much to be desired, however, because, having no preoperative data, she made comparisons with a control group of former state hospital patients who had experienced spontaneous recoveries. No data were given regarding time elapsed from date of operation (and for corresponding elapsed time for the controls, whatever the basis for such correspondence might be). Smallness of the sample, as well as insufficient evidence for the comparability of the groups, leaves one hoping that better data on these interesting techniques might some day become available. The issues may be summarized as follows:

Some patients who undergo standard "blind" lobotomies and also some in whom agranular portions of the frontal cortex are interfered with may show long-enduring or even permanent intellectual damage. This is all the existing evidence for deterioration. For the rest, it is inferred on the basis of inadequate evidence. Some of the inferences emanate from the untenable hypothesis that the operation should return the patient to his premorbid level of functioning. We do not demand that the heart of a cardiac, who has had a heart disease for five years and then undergone an operation, be as good as new. There is equal reason not to expect the mind of the chronically ill schizophrenic to be like new after operation; for example, even those patients who have recovered spontaneously show "scarring." In summary, the evidence points distinctly to the frontal lobes as mediating anxiety, but there is no evidence for their mediation of intelligence. To be sure, the distinctions between affect and intellect may be academic only, and it may be claimed that what appears to be an emotional defect is basically intellectual and vice versa. We do, however, have tests of intellect and these, under proper testing conditions, show no defect. We do not have adequate tests of anxiety, and that may be why the issue is not resolved.

As to the non-intellectual defects claimed to follow psychosurgery, Greenblatt and co-workers (18) pointed to excessive appetite and irritability as occurring in at least 50 per cent of the cases for whom information on these points was available. Whether these were transient or not was not indicated. Other defects reported were: outspokenness, lack of initiative, restlessness, and deficiencies in insight and in calculation.

Meyer, in *Perspectives in Neuropsychiatry*, (53) stated

> it has been possible to prove that a frontal lobe deficit syndrome may definitely occur if the lesions are entirely confined to the prefrontal region and that it may persist after the wound has become quiescent and unlikely to exercise an irritating effect over adjacent or remote parts of the brain.

But such defects may well occur in spontaneously improved patients and may be the residua of the psychoses rather than the sequelae of the operations. It is also noteworthy that two of the transient sequelae, urinary incontinence and convulsion seizures, were found to occur quite frequently pre-

operatively in this group of patients. In fact, the number of patients exhibiting these defects postoperatively for the first time was far smaller than the number exhibiting them preoperatively (18).

The frontal lobes are not the only parts of the cortex to be subjected to clinical investigation by means of surgical intervention in recent years. The thalamotomy operation which attempts to destroy the medial nucleus of the thalamus directly, and thus eliminate the thalamocortical connection, has been undertaken with reportedly good results. The temporal lobe has been operated on by Obrador (42a) and by Green *et al.* (17a) with some reported success. In the latter study, 23 patients suffering from psychomotor epilepsy underwent a temporal lobectomy, anterior to the acoustic receptive cortex. This operation seemed to be more effective for these patients than either frontal lobotomy or gyrectomy. Of the 23 patients, 12 had no more psychomotor seizures postoperatively. On all but one of these patients, the operation was unilateral, often on the dominant side. The patient with the bilateral anterior temporal lobectomy was not

> changed appreciably by the procedures, except that there is now an increased "hypermetamorphic impulse to action," loss of social consciousness and increased sexual interest—a modification of the results observed in monkeys following bilateral temporal lobectomies by Klüver and Bucy. His parents feel, however, that the loss of viciousness compensates for the factors mentioned above (17a).

The role of the parietal cortex on both sides of the brain in the proper functioning of spatial orientation has been demonstrated by a number of first rate observations according to Jefferson (29). Martin

> has considered that man's spatial orientation depends on the lower parietal field. Lesions in the parietal area have been shown to produce not only a loss of the power to find one's way about, even in the familiar purlieus of one's own house, but a loss of power to conceive space in even two dimensions. It becomes impossible to draw the plan of a house; for instance the kitchen may be depicted as a separate disconnected object in the garden. The same lack of orientation holds for dressing if the plan of articles of clothing can no longer be perceived. Concurrently may be seen a loss of recognition of half the body as a personal property (38a).

Behavioral studies such as this one, as well as those performed on cases with occipital lesions, indicate that the observant clinician can make contributions that could have emerged neither from experiments on animals nor from purely histological studies.

Another interesting suggestion arising from these investigations is that, contrary to popular opinion, consciousness itself is less dependent on the cortex than on the brain stem. The relevant area seems to be the upper pons and lower midbrain, presumably by some suppression of afferents or effect on the upper brain stem (38a).

Alterations in the Internal Environment

Richter (53) pointed out that biochemistry has moved on from static analysis of excreta to dynamic analysis of tolerance tests for various types

of stress. The influence of stress on the internal environment has joined biochemistry to psychology by a strong bond which augurs well for the future. Glucose tolerance tests, enzyme activity tests (5), dietary factors in the production of experimental neuroses (61), inadvertent production of canine "hysteria" by use of chemical "flour improvers" (39), glutamic acid studies (60), production of psychoses by atabrine (19, 47) and by mescaline (27), are some of the direct links between the dynamics of biochemistry and the dynamics of behavior. Whether the reduction in glucose tolerance of the schizophrenic is a cause or an effect of the psychosis, and whether the experimental neuroses and experimental psychoses are identical with the classical forms of spontaneous neuroses and psychoses are still moot questions, but the observation of these relationships and their quantitative evaluation cannot help but redound to the progress of abnormal psychology and physiology.

It is well known that the human brain obtains its energy from the oxidation of glucose at a rather low temperature with the help of enzymatic catalytic agents and that fully 25 per cent of body metabolism occurs in the brain (14a, 25). The oxygen is obtained directly from outside the body while the glucose and enzymes, though not imported from outside, depend directly for their production on synthesis of ingested substances. Unlike the rest of the organs of the body, the brain cannot avail itself of all the nourishment contained in the blood stream, since the blood brain barrier excludes certain substances. The enzymes are utilized but slowly, and need no continuous source of replacement, but the glucose and oxygen are used up so rapidly that any discontinuity in the supply of these elements makes itself felt immediately in behavior. The most dramatic effect of an interference in brain metabolism is loss of consciousness, but less drastic effects in the form of lower vigilance or performance, can be detected even when the interference is not very great.

Himwich (25) reviewed the present status of research in brain metabolism with reference to disorders of behavior. He stated that in order to get an effective measure of brain metabolism two factors must be determined: (*a*) the volume of oxygen absorbed by the brain from each unit of blood entering it (the arterio-venous oxygen difference), and (*b*) the total volume of blood flowing through the brain per unit of time (cerebral blood flow). When these two values are determined under basal conditions (during a quiet and restful interlude), the value of the cerebral metabolic rate (CMR) can be computed. This value represents the minimal amount of energy required for maintenance of brain function. Thus far, no gross differences have been found between schizophrenics and normals in cerebral metabolic rate. An anomaly has been noted, however, in avitaminosis leading to the accumulation of pyruvic acid, one of the intermediary products of carbohydrate metabolism. Deficiencies in vitamin B, especially thiamine, result in a diminished metabolic rate which produces definite behavioral changes as reflected in in the Porteus Maze score (44). When the availability of glucose is reduced

by insulin, reduction in cerebral metabolic rate of 40 to 56 per cent from normal must occur before contact with the environment is impaired. The lowest level of CMR compatible with maintenance of life and return of consciousness is 25 per cent of normal. Only two psychoses are found to be characterized by an abnormally low CMR—cerebral arteriosclerosis and general paresis. Lobotomy tends to lower the CMR up to three months after operation, but observations for longer follow-up periods are not yet available. The CMR is also found to be low in mongolian idiocy and in cretinism, but not in idiopathic mental defectives. In general, the CMR can not be raised above its normal level except in fever therapy, but it is correlated with neuromuscular activity, especially with convulsive seizures.

Another advance in understanding brain biochemistry comes from the recent application of the quick-freeze method to the fixation of tissue in experimental animals by plunging them into liquid air. This method has permitted a more precise analysis of biochemical processes under conditions of sleep, narcosis, convulsions, traumatic shock, and emotional excitement. Lactic acid is found in lower concentrations in brain tissue during sleep and narcosis than during the waking state, and in higher concentrations during emotional excitement, even when the animal is immobilized (53). Nitrogen metabolism is increased during convulsive seizures and the sensitivity of the brain to ammonia may be the actual cause of the seizure (53).

The effect of semistarvation on behavior was studied by Keys *et al.* (33) in the Minnesota inanition experiment conducted with volunteer "conscientious objectors." In their first volume the problems of morphology, biochemistry, and physiology are dealt with, and in the second volume psychology and some special medical problems are taken up. Despite the important behavioral changes accompanying starvation and despite the availability of many incidental field studies provided by nature in the form of various catastrophes, the authors found little to guide them in the design of their experimental study of starvation. The few experimental studies that were available seemed to be based largely on "fasting geniuses" who had some preconceived idea of the benefits of fasting, and thus introduced motivational elements which were not inherent in the starvation itself. In addition, the experimental situation itself is not altogether comparable to starvation in a "natural" setting with its emotional tone, uncertainty of survival, and the accompaniments of brutality that seemed to have been an indigenous feature of starving communities during World War II. Furthermore, the voluntary nature of the experimental starvation introduces additional problems in the form of conflict and required self-control in the presence of food. Despite these uncontrolled factors, the results of field studies of natural starvation, according to the authors, do not differ radically from the experimental studies reported in the Minnesota inanition experiment. The symptoms of depression, irritability, nervousness, general emotional instability, social withdrawal, narrowing of interests, obliteration of sexual

drive, and difficulty in concentration found in the Minnesota experimental subjects are closely paralleled in men suffering from natural starvation in the field studies examined. The concentration of attention on food, slowness of speech, and maintenance of memory and thinking functions on an unimpaired level are found equally often in both types of studies. It is the authors' contention that the Minnesota study may throw light on those questions on which field reports are silent or inadequate. Plausible though this contention may be, it is, nevertheless, of doubtful validity since it assumes complete comparability between experimental and field studies, which has not yet been demonstrated.

Some of the specific findings are rather striking. The relative immunity of dream life to starvation influences as demonstrated by the absence of any increase in food-dreams during the semistarvation period and the absence of any general effect on the frequency of dreaming is quite noteworthy. One suspects that there must have been rather wide individual differences in this respect and a further analysis of these data would be of great significance for the dynamics of dream behavior. Another possibility not to be neglected is that under conditions of semistarvation dreams are sooner forgotten, or are differentiated from daydreaming less readily. At all events, the striking influence of mild food deprivation on food imagery (36) is not confirmed by the Minnesota experiment.

The expected increase in the number of complaints during the semistarvation period and its decline during the rehabilitation period unfortunately is presented without statistical tests of significance. Presumably these differences ought to be and are significant, but no quantitative evaluation of the evidence is afforded.

Another striking finding is that despite complaints of lack of alertness during the semistarvation period, no significant difference in intellectual performance was acknowledged by the authors, although the data for the CAVD tests did show a statistically significant difference between the control testing and the 24th week of starvation. One wonders why an attempt was not made, either through control data on comparable subjects or through analysis of covariance methods (e.g., holding the control level constant and comparing the 12th and 24th weeks), to determine the significance of the differences. The cavalier dismissal of the obtained significant differences as due either to practice effects or to differences in difficulty of alternate test forms is not very convincing to the reviewer. The evidence from the parallelism of the learning curves for crossing out 4's (36, p. 862) seems irrelevant to the issue of whether intellectual performance is affected by semistarvation.

The use of personality inventories with the volunteer normal subjects of the type utilized in the Minnesota inanition study seems quite appropriate and the doubts regarding validity are not as strong in this situation as they might be in such situations as screening for admission to college or in-

dustry. Nevertheless, the absence of control data for testing the effect of repetition of tests is rather a severe handicap in the interpretation of the data. The utilization of the standards provided by the Minnesota Multiphasic Personality Inventory (MMPI) norms are indeed valuable, but based as they are on a rather wide sampling of individuals differing considerably in age, sex, educational level, etc., they cannot provide adequate controls for the group under study.

The lack of sensitivity of the projective techniques to semistarvation is not unexpected, since these techniques are so deficient in reliable scoring methods that even differences that might be present would be obscured.

That the changes in personality brought about by semistarvation are primarily in the neurotic, rather than in the psychotic sphere seems a reasonable expectation, and is not contradicted by the data. Neurotic depression and apathy seem to be the chief concomitants of semistarvation. It might be well to point out at this juncture that the terms "deterioration," "depression," and "apathy" as used by the authors do not correspond to the usual clinical terminology in psychopathology. Since the psychopathological meaning of these terms is so well established, it is unfortunate that they have been diverted to other uses. In general, this study will stand as a landmark among studies in the alteration of the internal environment and its concomitant effect on behavior. It is regrettable, however, that insufficient control data were presented and that attention was not paid to the effect of initial pre-stress level of functioning on differences obtained during successive testing under stress of semistarvation. The regression effect involved in such comparisons has been amply pointed out in recent years.

Some of the changes that occur under starvation are apparently aimed at maintenance of homeostasis; others are definitely the result of deficiency in substances required for adequate functioning. This holds true equally well in the physiological and the psychological fields. The presence of changes whose aim is maintenance of homeostasis probably reflects the survival value of such changes through the evolutionary process.

A number of sensory functions were also examined in the Minnesota experiment. The following visual functions were investigated: visual threshold, flicker fusion threshold, and perception of fluctuations in an ambiguous figure. Of these, only the flicker fusion threshold showed a statistically significant drop under conditions of semistarvation. It is somewhat paradoxical, however, to note that the authors dismissed the statistically significant difference in flicker fusion as biologically unimportant. This tendency to minimize differences is characteristic of the authors' evaluation of the entire project. To the reviewer, the problem of biological significance is not a value judgment to be made on the basis of preference, but a factual judgment to be based on understanding of the mechanism. Since flicker fusion has been shown to respond to alterations in brain metabolism (16a), it is quite understandable why the threshold should drop under conditions of semistarvation.

The fact that it dropped only one cycle seems to be an inadequate basis for judging its importance, since the actual biological value of a cycle has never been determined. Speed of tapping was also lowered by semistarvation, as were manual speed and coordination.

Another source of information about psychological concomitants of disturbed internal environments comes from the field of shock therapy. The psychological consequences of electric shock therapy have intrigued psychologists since the technique was introduced. Janis (28) conducted an intensive study by comparing 17 patients who received shock therapy with 17 matched controls. In previous studies (66), the influence of electric shock therapy on newly acquired (implanted) associations had been determined. Then investigations were made of the influence of shock on the free association process (65), that is, whether shock interferes with the old memories that are usually evoked by free association. Janis repeated the word association investigation, but extended his study to personal memories elicited by interview methods and to evaluation (by use of scaling methods) of the patient's anxiety, attitude toward specific disturbing memories, feelings of personal inadequacy, and other psychological symptoms. In general, although temporary changes were noted, no permanent losses of memory were found, but a definite decline in anxiety occurred. The role of the transient memory loss in the recovery process is still not determined. Theories range from regarding this memory loss as a secondary phenomenon of no possible consequence to the therapeutic process because it is not observed to the same extent in all patients, to the theory that the loss in the feeling of familiarity engendered by shock therapy may play an important role in defending the patient temporarily against emotionally toned memories.

Until comparative studies are made of memory function in improved patients who undergo other forms of therapy (psychosurgery, insulin shock, and other less drastic varieties of electric shock) as well as those who undergo psychotherapy or improve spontaneously, the role of memory loss as an explanatory principle in improvement will remain in doubt.

Behavioral Accompaniments of Life-Stress

The symposium on "Life Stress and Bodily Disease" (8) was a monumental contribution to the problem of psychosomatic medicine. Stimulated by Selye's hypothesis of the adrenal cortex as the central mediator of response to stress (56), spearheaded by recent developments in the adrenocorticotropic hormone (3, 50), and based on the painstaking observations of individual patients such as the case of "Tom" (63), the entire field of psychosomatics has taken on added scientific stature. According to Wolf, (8, p. 1090) in the closing remarks in the symposium:

> The common denominator in psychosomatic illness is the interpretation of an event as threatening. This implies anxiety, conscious or unconscious, and the need to

formulate a protective reaction pattern. The cause doesn't necessarily matter much. Neither does the reason for the selection of the particular pattern. The important thing is to substitute a less costly and more constructive pattern, or to remove the need for it. This may be accomplished if the event can be made no longer to seem threatening, or if this anxiety producing situation can be outweighed in the mass action equation by satisfactions and securities.

Most pertinent things for future study besides mechanisms are factors which evoke and modify patterns and ways of making them unnecessary. This approach may eventually illuminate the causes.

With this particular hypothesis in mind, the influence of events perceived as threatening was investigated by various workers on the functioning of the following body systems and organs: gastrointestinal system, genital system, cardiovascular system, arterial and renal blood flow, the eye, the "airways," the skin, the skeletal muscles, and so on. Instead of the analogical thinking which has pervaded psychosomatic investigations in the past, a good number of reports in this volume attempted to present measurable data on the various bodily responses which accompany stressful situations. The result is a rather overwhelming mass of material supporting the hypothesis of threat from which the present review can include only certain selected portions.

Wolff (64), who organized this symposium, pointed out that there are two main sources of threat to the integrity and well-being of individuals: (*a*) the physical environment (other forms of life or cosmic forces), and (*b*) the internal symbolic environment (threats of danger arising from past experience, dependency feelings, and conflicts between drives). It is important to realize that not only actual threats, but potential threasts may serve to mobilize the individual's defenses. Hence, not the "real" universe, but the "perceived" universe is the source of the threat. Once a threat appears, defense mechanisms are set into action involving both bodily preparations and certain feelings and attitudes. Usually one organ or system becomes characteristically involved in this defense action, and it is the continued involvement of this organ long after the threat episode has disappeared that constitutes the psychosomatic problem. Why one organ rather than another becomes involved is still unknown.

The studies reported in this book range from an examination of the developmental life history of the patient, à la Adolf Meyer (42), to a minute investigation of the organ involved in the particular psychosomatic condition under investigation. The entire armamentarium of the clinical psychologist, psychiatrist, and physiologist was brought to bear on the question of psychosomatic deviations. Fluctuations in the course of the psychosomatic illness were correlated with events, attitudes, emotions, and behavior of the subject. Whenever a positive correlation between the course of the illness and some complex situation became evident, a short term experiment was carried out to determine the quantitative manner in which the affected organ

responded to the life situation in question. This experiment was repeated after a suitable control period of relative relaxation to see whether the event or episode would again evoke the deviant activity of the bodily organ.

The specific procedure of the analysis was as follows: Whenever a life situation seemed to be associated with say, a gastric disturbance, it was in a subsequent period retested by bringing up the suspected conflicts for discussion. If this maneuver reproduced a gastric disturbance, and if it could be turned off by inducing relaxation in the subject by strong reassurance, the validation of the suspected relationship between the emotional disturbance and the gastric disturbance was considered settled. One wonders whether the rapidity with which the changes occurred are reflections of rapid change in mood and attitude or of some other factors which intervened. It hardly seems likely that the rapid changes reported could actually be concomitants of psychological changes in the individual.

It should be noted, however, that life stress alone, or its reduction, is not the only agent which can bring about alterations in body functioning. Some hyperfunctioning or hypofunctioning of an organ, regardless of how it arises, may regain its proper level of functioning spontaneously without, or in spite of the intervention of life situations. To expect that every waxing or waning of organ functioning would depend upon or be responsive to life situations, or that there is a rigid convariation between the two, is at the present time merely a wishful hypothesis rather than an established fact.

The relative importance of unconscious and conscious processes (38, 62) in producing conflicts that affect the functioning of bodily organs was also discussed. The notion that only unconscious conflicts are effective in producing bodily changes does not seem to be tenable since, at least in the case of "Tom," stress and conflict that had the most significance was of the variety that was the most threatening for the individual regardless of whether it was at the conscious or unconscious level. To be sure, it has been claimed that conflicts that have become unconscious are of greater significance to the individual, but the fact of lack of awareness per se is not the important factor in producing a bodily change.

Following the psychodynamic principles proposed by Alexander (1) regarding the role of emotional factors in gastric dysfunction, data are presented to indicate that there are at least two contrasted patterns of gastrointestinal reaction to stress: (*a*) gastric hyperfunction in which the subject behaves as if he were about to be fed and (*b*) an ejection-riddance pattern in which the subject behaves as if he had been poisoned, reacting with sudden cessation of gastric digestion, nausea, and vomiting.

Feelings of anger, deprivation, longing for emotional support, or need of being cared for, give rise to hyperfunction of the gastrointestinal system in about the same way that hunger evokes such hyperfunctioning. It is suggested that these aggressive patterns have become conditioned to gastric hyperfunctioning as a result of the early aggressive patterns that manifest them-

selves in the infant when he is hungry. The feelings of threat arising from food deprivation may later arise or be stimulated when any other threatening situation hovers over the individual. The conditioned response of salivation and gastric activity which originally was associated with this aggressive hunger-behavior pattern may therefore lead to hypersalivation and hyperfunction in later life when threatening situations occur. This gastric hyperfunction is usually associated with increased blood flow, motility, and acid secretion rendering the mucous membrane quite fragile. That there may be cortical structures involved in this type of activity is also suggested by the work of Davey, Kaada & Fulton with monkeys and dogs (13).

The ejection-riddance reaction involving the large bowel, stomach, and duodenum, which have the protective purpose of ejecting from the body undesirable agents such as noxious foods or poisons, may also operate when the individual is under the influence of certain undesirable emotional situations, even when no noxious agent of the physical variety is present. Studies of this reaction have indicated that there are two patterns of gastric hypofunction: (*a*) a pattern associated with overwhelming catastrophic feelings of fear, terror, horror, abject grief, depression, and despair, in which practically all gastric function comes to a standstill since gastric productivity is futile in the face of the overwhelming catastrophe and (*b*) a pattern associated with feelings of disgust and contempt, in which gastric function is also reduced, but mucous production is increased, as is also duodenal motor function and the concomitant skeletal muscle contraction which leads to vomiting. Here the organism behaves as though the noxious incident inadvertently "ingested" could be diluted, neutralized, and ejected.

Protective reaction patterns to threats designed to mobilize the entire organism for action are often associated with constipation, since during such bodily preparations for action, emptying of the bowel seems useless. It has been noted that situations evoking sadness, dejection, or cheerless striving inhibit the gastro-colic reflex, induce nonpropulsive phasic contractions in the sigmoid, and interfere with the mass reflex.

> It is as though the individual, unable to face and grapple with the threat, but nevertheless firmly holding on, is tensely waiting for an attack which is indefinitely delayed. The animal so poised is not in the state optimal for defecation (2).

Another psychosomatic focus is found in the circulatory system (49). Usually, the augmented response of the circulatory system to the standard exercise situation dies off in about 2 min. but in cases of individuals under stress, the response persists much longer. Thus, normal individuals who are confronted with tasks which they do not relish or which they are convinced they cannot perform successfully, exhibit in response to the standard exercise test a striking increase in blood pressure and stroke volume and a decrease in ventilatory efficiency. This augmented response may persist for 48 hr.

Life stress in individuals with essential hypertension is capable of producing changes in the direction of renal ischemia which may eventually lead to renal damage.

One of the most speculative and yet intriguing hypotheses proposed in this *Life Stress and Bodily Disease* volume is that of Hinkle (26), who suggested a psychosomatic basis for diabetes. In cases of starvation, it is well known that the body generally tends to limit itself to a noncarbohydrate metabolism, reserving the available carbohydrates for brain and cord metabolism, since the latter cannot utilize any other source of nourishment. Arguing from this fact, Hinkle suggested that not only actual starvation, but also threats of starvation arising from removal of mother love and bodily security may give rise to a limited carbohydrate metabolism. In later maturity, some individuals respond to stresses involving withdrawal of love and security as if they were threats of starvation. For short durations such responses may be harmless, but if continued for a long time irreversible changes characteristic of diabetes may occur.

Similarly, psychosomatic involvements of the "airways," of vascular changes producing headaches, and other deviant functioning of bodily organs can be explained on the basis of the threat-response hypothesis.

The severest stricture that can be drawn up against the threat-response hypothesis in psychosomatic disorders is that such stressful events probably occur in the life experience of most individuals, yet everyone does not develop a psychosomatic illness. It seems as if a predisposing factor is essential before the illness will take root. Whether this predisposing factor is a genetic one, or whether it is derived from life experience, cannot be determined at present.

A suggested confirmation of the threat-response hypothesis is provided by the dramatic disappearance of psychosomatic illness when a new threat, overshadowing the former threats arises. Since it is the response to "perceived" rather than to "actual" threat that is the important basis for the development of the illness, it becomes explicable why migraine headaches, for example, were completely lost in missionaries incarcerated in a Japanese concentration camp.

An interesting effect of culture on psychosomatic conditions is afforded by the suggestion that each culture provides anxiety-reducing agents or institutions which mitigate the threat-potency of a given event (57). As long as the individual adheres to his cultural mores his anxieties are reduced by the institutions created for his protection. When, however, as a result of a breakdown in his cultural allegiance he ceases to adhere to his mores, he is more prone to become subject to psychosomatic conditions, since the threat-potency remains while the anxiety-reducing agent loses its effect. This is a plausible argument, but one in need of factual verification, since, for example, occidentalized individuals come under closer medical scrutiny than primitive individuals, and the greater frequency of psychosomatic illness in

the westernized individuals may be based merely on fuller information and greater contacts.

The association between urticaria and resentment (17) was demonstrated in 30 unselected cases. In the discussion, however, Sulzberger pointed out that, in his experience, lasting skin lesions produced by psychogenic mechanisms alone never occurred. In most instances it was the patient's rubbing or scratching the spot on the skin that seemed to precede the occurrence of the lesion. Only in rare cholinergic conditions does a lesion appear without scratching or rubbing.

Direct evidence bearing upon the specific involvement of a given organ in stress is afforded by the investigation of the stress situation produced by the Hardy-Wolff-Goodell pain stimulator, which can yield quantitative degrees of thermally-produced stress (37). Recordings were taken simultaneously of muscle potentials of the left side of the neck, of heart rate, and of respiration. The results of this study indicate that there is considerable specificity in the effect of a stress situation and that those organs concerning which the patient complains seem to be the ones that are more sensitive to stress. Thus, patients with cardiovascular complaints show deviations from those without cardiovascular complaints in mean heart rate and its variability, and in the median respiratory variability, while the patients whose symptoms included complaints of head and neck pains showed higher muscle potential scores than those who had no such complaints. On the other hand, considering patients with cardiovascular complaints, as opposed to those who had no cardiovascular complaints, no difference was observed with regard to muscle score. Similarly, patients whose chief complaints were head and neck pains, as contrasted with those who had no such pains, showed no difference with regard to heart rate or respiration scores. The authors concluded that psychiatric patients with somatic complaints tend to manifest increased physiological responsiveness in the related physiological system or mechanism upon exposure to stress. This holds true even when the subjective symptom that the patient complains of is not experienced at the time of stress.

An attempt was made to relate such well-known emotions as anxiety, tension, resentment, depression, and elation, with the blood content of the individual undergoing these emotions (14). The method consisted of determining through psychiatric interview the type of emotion experienced. A blood sample was taken during the interview, and the effect of this blood on isolated animal tissue of known reactivity was determined. The results indicated that blood taken during an interview in which tension was the most characteristic emotional state seemed to have effects on the tissue indicators similar to that produced by acetylcholine. Resentment was found to resemble atropine in its effects (i.e., atropinized tissue is refractory to acetylcholine) and anxiety was found similar to epinephrine. This is an interesting attempt at getting a diagnostic method for determining the type of

emotion present. It is, however, still questionable whether the type of emotion observed psychiatrically can be sufficiently well diagnosed to be reliably labeled. It is likely that until we are able to elicit genuine emotions to order in the laboratory, the problem of finding the correlates of a given emotion will remain unsolved. Nevertheless, the fact that consistent differences are obtained between blood samples of people who allegedly are undergoing different emotions does indicate that the foregoing attack may in the end prove to be very fruitful.

In a study of patients suffering with reflex sympathetic dystrophies, characterized by vasomotor instability and having causalgic (agonizing or burning pain) symptoms, Ruesch & Prestwood (55) found that these patients suffered from a primary disorder in communication. Their mastery of verbal expressions and gestures was found to be inadequate and concrete, and their interpersonal communication was replaced by intrapsychic or self-referred communication. This tended to make these patients appear immature and "difficult" in therapy. One wonders whether the particular type of immaturity found in these patients is specific to the disease or whether it is a general characteristic of psychosomatic illness. Until adequate psychological tests are administered, one may even question the basis for attributing their faults in communication to the disease process. The smallness of the sample casts further doubt on the hypothesis. Nevertheless, it is intriguing and worthy of follow-up.

Persky, Greuber & Mirsky (48) found that the excretion of hippuric acid is related to "free" anxiety, rising with increase in anxiety and dropping when the anxiety level decreases. This relationship was found to hold true only of patients and not of normal students anticipating an examination. They also found that sympathomimetic substances (atropine, ephedrine, and l-norepinephrine) lower hippuric acid secretion. The authors claimed that psychosurgery tended to reduce hippuric acid secretion in two patients suffering from anxiety (agitated paranoid schizophrenia and recurrent psychotic depression) and elevated it in two patients showing withdrawal (catatonic schizophrenia and paranoid schizophrenia). A careful examination of these data (48, Table 33) as well as of those on the effects of psychotherapy (48 Table 30) and of drugs (48 Table 31), casts doubt on the validity of the claims, since there is quite a distinct regression effect. Those of high initial level show a drop and those of low initial level show a rise. Until patients with equal initial levels in the contrasted groups are examined, no conclusive results will be available. Despite this shortcoming, the data presented are provocative, and the quest for a biochemical analogue for anxiety is brought a step nearer to its solution.

Genetically-Influenced Disorders

Most clinical delving into the past of an individual stops at the womb, as Slater (58) pointed out, and at least an alleged explanation of his life history

can often be found without resorting to genetically based factors. The elimination of genetic factors from a consideration of man's behavior, however, would tend to single him out as unique from the rest of the animal world.

The controversy between hereditarians and environmentalists is fast disappearing as a basic issue. Only one distinction still separates the two camps. While the environmentalists regard all human beings as potentially susceptible, hereditarians insist that only those who have a predisposition for a given mental disorder will develop it. Thus, while everyone will admittedly respond with a temporary seizure if a sufficiently strong electrical stimulation is applied, only the basically epileptic may continue having seizures after the electrically induced stress is no longer operative.

The techniques which have been evolved for answering the question of the relative importance of heredity and environment consist essentially of the following procedures: (*a*) investigation of the incidence of a given disease in blood relatives and in pairs of identical and fraternal twins, separated into those reared together and those reared apart (31); (*b*) investigation of the incidence of disease in blood-siblings as contrasted with foster-siblings (10); (*c*) investigation of the role of intercurrent disease during pregnancy and other prenatal rather than hereditary factors (9); (*d*) study of the rate of myelinization and its effect on resistance to disease (32); (*e*) investigation of the developmental history and of critical junctures in this development (42); and (*f*) studies in genetic linkage (20). The work of Kallmann in the investigation of twins, at least one of whom is affected by a given disease, is too well known to require introduction. He has recently (30) extended his methods to the investigation of suicide, for which he finds no hereditary factor, to manic depressive psychosis, and to diseases of the senium. Critics of his methods may point to possible selective factors in diagnoses, or appeal to the importance of early life deprivations of a physical or mental variety; nevertheless, his data are overwhelmingly convincing of the importance of genetic factors in the causation of mental disorders. Furthermore, his plea for a study of the environmental differences operating in cases of identical twins who are found discordant with regard to schizophrenia is well taken and such studies ought to cast considerable light on etiological factors.

Among the newer trends in research is the attempt to map the genes on each chromosome in man in the same way that such mapping has been carried out in the fruit-fly (20). There are now about eight chromosomes which have easily identifiable markers—sex chromosome, several blood factors, and taste for phenylthiocarbamide. Slater suggested that if linkage could be established between any of these easily identifiable markers and a given disease, e.g., Huntington's chorea, prediction of the occurrence of this disease, even before it became evident, would be possible (58).

Perhaps the most thorough study of the claims of the environmentalist in the causation of personality disorders was made by Orlansky (43). After reviewing 149 studies in this area, culled from the publications of psychol-

ogists, psychiatrists, and anthropologists, he concluded that most of the evidence for the influence of infant care on adult personality forces one to a negative conclusion. Apparently other factors in addition to mere infant experience are important in the development of the adult personality. Among these, the role of genetic factors and still undetected physiological and biochemical factors in growth and development loom large. One wonders whether the type of material care given an offspring is not itself also relatively independent of the care which the mother received as an infant, and more dependent upon the genetic, physiological, and biochemical structure of the mother herself. The recent investigations of the relationship between the Rh factor and mental deficiency, rubella and mongolian idiocy, and nutritional deprivation and subsequent maldevelopment may lead to the isolation of disease-producing factors for illnesses which now are still in that unhappy borderline between psychogenic and physiogenic theory.

LITERATURE CITED

1. Alexander, F., *Psychosomatic Medicine* (W. W. Norton & Co., Inc., New York, 300 pp., 1950)
2. Almy, T. P., Kern, F., Jr., and Abbot, F. K., in *Life Stress and Bodily Disease*, 724–31 (Proc. Assoc. Research Nervous Mental Disease, 1949, Williams & Wilkins Co., Baltimore, 1135 pp., 1950)
3. Altschule, M. D., *Bull. New Engl. Med. Center*, **11**, 229–34 (1949)
4. Anglo American Symposium on Psychosurgery, Neurophysiology, and Physical Treatments in Psychiatry, *Proc. Roy. Soc. Med., Sect. Psychiatry, Suppl.* **42**, 95 pp. (1949)
5. Ashby, W., *J. Nervous Mental Disease*, **105**, 107–15 (1947)
6. Ashby, W. R., and Bassett, M., *J. Mental Sci.*, **95**, 418–30 (1949)
7. Assoc. Research Nervous Mental Diseases, *The Frontal Lobes* (Williams & Wilkins Co., Baltimore, 901 pp., 1948)
8. Assoc. Research Nervous Mental Disease, *Life Stress and Bodily Disease* (Williams & Wilkins Co., Baltimore, 1135 pp., 1950)
9. Aycock, W. L., and Ingalls, T. H., *Am. J. Med. Sci.*, **212**, 366–79 (1946)

9a. Bailey, P., "Concerning the Organization of the Cerebral Cortex," *Texas Repts. Biol. Med.*, **6**, 34–56 (1948)

10. Burks, B. S., and Roe, A., *Psychol. Monographs*, **63**(5), 62 pp. (1949)
11. Cameron, N., *Ann. Rev. Psychol.*, **1**, 189–206 (1950)
12. Congrès International de Psychiatrie, *Anatomo-Physiologie Cérébrale à la Lumière des Lobotomies et Topectomies*, **3** (Librarie scientifique, Hermann & Cie, Paris, 146 pp., 1950)
13. Davey, L. M., Kaada, B. R., and Fulton, J. F., in *Life Stress and Bodily Disease*, 617–28 (Proc. Assoc. Research Nervous Mental Disease, 1949, Williams & Wilkins Co., Baltimore, 1135 pp., 1950)
14. Diethelm, O., Fleetwood, M. F., and Milhorat, A. T., in *Life Stress and Bodily Disease*, 262–79 (Proc. Assoc. Research Nervous Mental Disease, 1949, Williams & Wilkins Co., Baltimore, 1135 pp., 1950)

14a. Folch, J., in *Psychiatric Research*, 27 (Harvard Univ. Press, Cambridge, Mass., 113 pp., 1947)

15. Freeman, W., and Watts, J. W., *Psychosurgery in the Treatment of Mental Disorders and Intractable Pain*, 2nd Ed. (Charles C Thomas, Publisher, Springfield, Ill., 598 pp., 1950)
16. Fulton, J. F., *Functional Localization in Relation to Frontal Lobotomy* (Oxford Univ. Press, New York, 140 pp., 1949)

16a. Gellhorn, E., and Hailman, H., *Psychosomat. Med.*, **6**, 23–30 (1944)

17. Graham, D. T., in *Life Stress and Bodily Disease*, 987–1009 (Proc. Assoc. Research Nervous Mental Disease, 1949, Williams & Wilkins Co., Baltimore, 1135 pp., 1950)

17a. Green, J. R., Duisberg, R. E. H., and McGrath, W. B., *J. Neurosurg.*, **8**, 157–72 (1951)

18. Greenblatt, M., Arnot, R., and Solomon, H. C., *Studies in Lobotomy* (Grune & Stratton, Inc., New York, 495 pp., 1950)
19. Grieber, M. F., *Am. J. Psychiat.*, **104**, 306 (1947)
20. Haldane, J. B. S., *Perspectives in Biochemistry*, 1–11 (Needham, J., Ed., Cambridge University Press, Cambridge, 1937)

21. Halstead, W. C., *Brain and Intelligence* (Univ. Chicago Press, Chicago, 206 pp., 1947)
22. Halstead, W. C., Ed., *Brain and Behavior, A Symposium* (Univ. California Press, Berkeley, 1950)
23. Head, H., *Brit. J. Psychol.*, **14**, 126–47 (1923)
24. Hebb, D. O., *The Organization of Behavior* (John Wiley & Sons, Inc., New York, 335 pp., 1949)
25. Himwich, H. E., *Brain Metabolism and Cerebral Disorders* (Williams & Wilkins Co., Baltimore, 451 pp., 1951)
26. Hinkle, L. E., Jr., and Wolf, S., in *Life Stress and Bodily Disease*, 338–90 (Proc. Assoc. Research Nervous Mental Disease, 1949, Williams & Wilkins Co., Baltimore, 1135 pp., 1950)
27. Hoch, P. H., *Am. J. Psychiat.*, **107**, 607–11 (1951)
28. Janis, I. L., *J. Nervous Mental Diseases*, **3**, 359–97, 469–89 (1950)
29. Jefferson, G., *Brit. Med. Bull.*, **6**, 333–39 (1950)
30. Kallmann, F. J., De Porte, J., De Porte, E., and Feingold, L., *Am. J. Human Genetics*, **1**, 113–26 (1949)
31. Kallmann, F. J., *Génétique et Eugénique*, **4**, 1–40 (1950)
32. Kennard, M. A., and McCulloch, W. S., *J. Neurophysiol.*, **6**, 181–89 (1943)
33. Keys, A., Brozek, J., Henschel, A., Mickelsen, O., and Taylor, H. L., *The Biology of Human Starvation* (Univ. Minnesota Press, Minneapolis, 2 vols., 1385 pp., 1950)
34. Landis, C., Zubin, J., and Mettler, F. A., *J. Psychol.*, **30**, 123–38 (1950)
35. Lewis, N. D. D., Ed., *Studies in Topectomy* (Nervous and Mental Disease Press, New York, in press)
36. McClelland, D. C., and Atkinson, J. W., *J. Psychol.*, **25**, 205–22 (1948)
37. Malmo, R. B., Shagass, C., and Davis, F. H., in *Life Stress and Bodily Disease*, 231–62 (Proc. Assoc. Research Nervous Mental Disease, 1949, Williams & Wilkins Co., Baltimore, 1135 pp., 1950)
38. Margolin, S. G., Orringer, D., Kaufman, M. R., Winklestein, A., and Levy, M., in *Life Stress and Bodily Disease*, 656–64 (Proc. Assoc. Research Nervous Mental Disease, 1949, Williams & Wilkins Co., Baltimore, 1135 pp., 1950)
38a. Martin, J. P., *Lancet*, **I**, 1–6, 48–53 (1949)
39. Mellanby, E., *Brit. Med. J.*, **II**, 885–87 (1946); **I**, 231 (1947)
40. Mettler, F. A., Ed., *Selective Partial Ablation of the Frontal Cortex* (Paul B. Hoeber, Inc., New York, 517 pp., 1949)
41. Mettler, F. A., Ed., *Problems in Psychosurgery* (The Blakiston Co., Philadelphia, in press)
42. Muncie, W., *Psychobiology and Psychiatry*, 2nd Ed. (C. V. Mosby Co., St. Louis, 620 pp., 1948)
42a. Obrador, A. A., *J. Neuropathol. Exptl. Neurol.*, **6**, 185–93 (1947)
43. Orlansky, H., *Psychol. Bull.*, **46**, 1–48 (1949)
44. O'Shea, H. E., Elsom, K. O., and Higke, R. V., *Am. J. Med. Sci.*, **203**, 388–97 (1942)
45. Partridge, M., *Prefrontal Leucotomy* (Blackwell Scientific Publications, Oxford, 496 pp., 1950)
46. Penfield, W., and Rasmussen, T., *The Cerebral Cortex of Man* (The Macmillan Co., New York, 248 pp., 1950)

47. Perk, D., *J. Mental Sci.*, **93**, 756–71 (1947)
48. Persky, H., Grinker, R. R., Mirsky, I. A., and Gamm, S. R., in *Life Stress and Bodily Disease*, 297–307 (Proc. Assoc. Research Nervous and Mental Disease, 1949. Williams & Wilkins Co., Baltimore, 1135 pp., 1950)
49. Pfeiffer, J. B., Jr., and Wolff, H. G., in *Life Stress and Bodily Disease*, 929–53 (Proc. Assoc. Research Nervous Mental Disease, 1949, Williams & Wilkins Co., Baltimore, 1135 pp., 1950)
50. Pincus, G., and Hoagland, H., *Am. J. Psychiat.*, **106**, 641–59 (1950)
51. Porteus, S. D., *The Porteus Maze Test and Intelligence* (Pacific Books, Palo Alto, Calif., 194 pp., 1950)
52. *Psychosurgery*, 1st International Conference (Livraria Luso-Espankola 86-R, Lisbon, 331 pp., 1949)
53. Richter, D. E., Ed., *Perspectives in Neuropsychiatry* (H. K. Lewis & Co., Ltd., London, 236 pp., 1950)
54. Robinson, M. F., in *Psychosurgery*, 2nd Ed., 314–28 (Freeman, W., and Watts, J. W., Eds., Charles C Thomas, Publisher, Springfield, Ill., 1950)
55. Ruesch, J., and Prestwood, A. R., in *Life Stress and Bodily Disease*, 211–31 (Proc. Assoc. Research Nervous Mental Disease, 1949, Williams & Wilkins Co., Baltimore, 1135 pp., 1950)
56. Selye, H., and Fortier, C., in *Life Stress and Bodily Disease*, 3–19 (Proc. Assoc. Research Nervous Mental Disease, 1949, Williams & Wilkins Co., Baltimore, 1135 pp., 1950)
57. Simmons, L. W., in *Life Stress and Bodily Disease*, 127–37 (Proc. Assoc. Research Nervous Mental Disease, 1949, Williams & Wilkins Co., Baltimore, 1135 pp., 1950)
58. Slater, E., *Génétique et Eugénique*, **4**, 119–53 (1950)
59. Taub, N., *Ann. Rev. Psychol.*, **2**, 217–38 (1951)
60. Waelsch, H., *Am. J. Mental Deficiency*, **52**, 305–13 (1948)
61. Williams, R. D., and Mason, H. L., *Proc. Staff Meetings Mayo Clinic*, **16**, 433–38 (1941)
62. Wolf, S., and Jersy Glass, G. B., in *Life Stress and Bodily Disease*, 665–79 (Proc. Assoc. Research Nervous Mental Disease, 1949, Williams & Wilkins Co., Baltimore, 1135 pp., 1950)
63. Wolf, S., and Wolff, H. G., *Human Gastric Function* (Oxford University Press, New York, 195 pp., 1943)
64. Wolff, H. G., in *Life Stress and Bodily Disease*, 1059–95 (Proc. Assoc. Research Nervous Mental Disease, 1949, Williams & Wilkins Co., Baltimore, 1135 pp., 1950)
65. Zeaman, J., *Effect of Shock Therapy in Performance on the Word Association Test* (Master's thesis, Columbia Univ., 1947)
66. Zubin, J., in *Methods of Psychology*, 595–623 (Andrews, T. G., Ed., John Wiley & Sons, Inc., New York, 716 pp., 1948)
67. Zubin, J., *Second Conference on Psychosurgery, National Institute Mental Health* (Govt. Printing Office, Washington, D.C., in press)
68. Zubin, J., Windle, C., and Hamwi, V., *J. Nervous Mental Diseases* (In press)

CLINICAL METHODS: PSYCHODIAGNOSTICS[1]

By Ann Magaret

Department of Psychology, University of Wisconsin, Madison, Wisconsin

Trends in the development of any field of investigation are defined as surely by omissions or underemphases as by inclusions and accentuations. The past year's publications in the area of psychodiagnostics illustrate this principle clearly. There are perhaps 300 books and papers, not all of which can be reviewed here, dealing with some aspect of psychodiagnostics. Over half of these are concerned with projective methods, their standardization, scoring, and success in differentiating clinical populations or in yielding diagnostic statements in psychiatric language. Of the remaining publications, the bulk report results of objective intelligence scales, applied either as complete or abbreviated intelligence measures or as instruments for diagnosing behavior disorder. Only a handful of papers on psychodiagnostics attack directly the highly important questions of research methodology, of aims and philosophy of diagnosis, or of explicit theoretical framework which supports one diagnostic method and not another.

The emphases and omissions indicated by this overview define the trend of the past year's work in psychodiagnostics—a trend noted in the first volume of the *Annual Review of Psychology* (98) and continued and intensified to the present (35). The modal study is still the investigation of diverse diagnostic groups by means of projective techniques, perhaps with more attention to the reliability of diagnoses than last year, and certainly with more refinements of scoring. The rare study is still the one which proceeds from explicitly stated personality theory to specific test of hypotheses through psychodiagnostic procedures. Typically, empirical findings are accentuated; theoretical formulation and research methodology are omitted or underemphasized.

It would be unjust to a great many recent papers to conclude from this analysis that workers in the field of psychodiagnostics are naïve either with regard to theory or to the appropriateness and power of various investigative methods. The point here is one of emphasis in approach and reporting. It is as if the clinician performs first the very real practical task of categorizing or describing his patients. Only later, often secondarily and briefly, he indicates the assumptions which have dictated his choice of method and refers to the larger body of personality theory to which his findings may be ordered. In such incidental ways as these, psychologists concerned with psychodiagnosis have this year made contributions which go beyond the pragmatic problem of accurate psychiatric diagnosis. Perhaps it is inevitable,

[1] The survey of the literature to which this review pertains was completed about May, 1951.

though still unfortunate, that the reader must search each publication with care before he comes upon these important developments.

The Problem of Psychodiagnosis

Diagnostic investigation presumably begins with a question regarding the patient and ends with a more or less precise answer which advances one's understanding of the patient as a person. The direction which diagnostic investigation takes depends largely upon the comprehensiveness of the clinician's goal. At present, it depends to a lesser extent upon the personality theory the clinician prefers. The goal may be as restricted as the locating of the patient within a conventional diagnostic group; or it may be as extensive as the prediction of what the patient will later do in laboratory situations, in therapy, or in future interpersonal relationships outside the clinic setting. The preferred theory may require the measurement of individual traits, the specification of needs, or the description of conflict among so-called "levels" of personality organization. There is currently wide diversity among psychologists in both the goals they accept for diagnosis and the theories they prefer.

Goals of diagnosis.—The problems presented by this diversity of approach come out clearly in the papers comprising the American Psychological Association's 1948 Symposium on Recent Advances in Diagnostic Techniques, published this year as part of the American Lecture Series (81). In his paper summarizing the symposium, Miller (134) calls attention to the trend away from diagnosis in Kraepelinian and Bleulerian categories. Dynamic description in terms of flux or process are slowly replacing the conventional psychiatric categories. Miller notes the restrictions which semantic disagreement places upon this development; this matter of restrictions is given explicit statement in a later study by Grayson & Tolman (70). The latter investigation shows plainly the extent of disagreement and consequent misunderstanding among psychologists and psychiatrists in the use of professional terminology in report writing. Miller notes as well the promise of factor analytic methods in increasing the precision with which diagnostic categories may be defined. Some indication of the fruitfulness of this approach is to be found in Wittenborn's factor analysis of 55 symptom scales (191), applied to nearly a thousand hospital patients, which yielded nine distinguishable syndromes.

Harris' contribution to the symposium (80) shows more concretely the consequences of this gradual shift in diagnostic emphasis. Using as illustrations several recently published research papers on psychodiagnosis, he contrasts neatly the limited "criterion group" method with a broader and more fruitful approach which follows current psychiatric thinking. The latter approach begins with a significant psychiatric concept, the concept of ego strength, for example. Harris points out convincingly that certain recent research findings may be interpreted in terms of ego strength rather than in terms of conventional psychiatric categories. Results obtained in selected

XUM

Rorschach inspection technique studies and in factor analytic investigations of objective personality tests converge in providing dimensions for the measurement of ego strength. Harris argues, equally persuasively, that, since the phenomena the diagnostician is trying to understand are typically interpersonal, the relevant variables to study are those with a social reference.

If the goal of diagnosis is shifting from that of sorting patients into categories, then one may fairly ask what other goals dictate the design and direction of recent research in psychodiagnostics. A substantial number of writers mention the accurate prediction of overt behavior as a desirable outcome of diagnostic investigation. In one of the few direct approaches to this problem, Luft (124) designed two experiments to measure the relative accuracy of clinicians and nonclinicians in predicting patient behavior. In the first experiment, judges listened to a case conference summary and attempted to predict the patient's responses on the Guilford inventory STDCR. In the second, the judges read a diagnostic interview and attempted to predict the patient's responses to Bell Adjustment Inventory items and to a sentence-completion test. More judges made extra-chance scores in the first experiment than in the second, but neither experiment showed a direct relationship between clinical training and ability to predict verbal behavior. It is of some interest that Luft was able to trace almost every prediction, correct or incorrect, to a working hypothesis held by the judge about the patient.

A more restricted study involving the prediction of overt behavior, this time self-ratings, is that of Calvin & Ward (28). Ninety subjects were exposed to combinations of five lights, which were sometimes, but not always, followed by a buzzer. On each trial, the subject stated whether or not the buzzer would sound and indicated his certainty of prediction on a five-point scale. The 10 most certain and 10 least certain subjects then took the Thematic Apperception Test (TAT). No striking differences between these two extreme groups in their TAT stories emerged, although the high-certainty group tended to construct central characters with strong needs for harm-avoidance, autonomy, and dominance.

These findings are reminiscent of the results obtained in the Michigan Veterans Administration Assessment research (107). A monograph by Tupes (178), based on the original Michigan data, provides information regarding the relationship between material obtained from initial and intensive interviews and final pooled ratings on a number of personality traits. The initial interviewer had relatively little information about the subject; the intensive interviewer had access to the subject's credentials, autobiography, objective test scores, and projective protocol material. The results indicate that interviews make only a slight contribution to the validity with which the personality variables are rated. Although the intensive interviewer makes somewhat more valid ratings than does the initial interviewer, the difference in net gains between the two is not statistically significant. The intensive interview ratings, based on comprehensive psychological data and a 2-hr. "probing" interview, yield ratings whose median correlation with the final

criterion ratings is about .63, a figure which Tupes notes is not particularly high for individual prediction.

As might be expected, if the goal of diagnosis is a broad, general predictive statement, rather than the specification of what the patient will do in concrete situations, then the accuracy of prediction is likely to be greater. Wittson & Hunt (99, 100, 193), in evaluating the validity of brief psychiatric interviews of large numbers of men coming to a naval receiving ship, support this point of view. Here the interviewer made simple judgments of the relative severity of symptomatology shown by his subjects. The subsequent psychiatric histories of the men provided information concerning the relative incidence of neuropsychiatric disability. There is a systematic and significant relationship between the graded severity of symptomatology and the incidence of subsequent neuropsychiatric discharge from service. In a similar fashion, psychodiagnostic techniques may provide general predictive statements regarding the outcome of therapy. As part of his contribution to the symposium described above, Muench (138) reviews eight studies in which psychometric instruments have been used with some success to determine the treatability of patients.

Four other papers, not experimental in character, attack directly the problem of prediction. DuMas (48, 49) describes two methods of validating predictive statements made by diagnosticians: the method of subsequent empirical measurement and the method of agreement with predictions made by a panel of "experts." From these methods, he attempts to state "formulas" for diagnostic proficiency. Rosenzweig (163) proposes a method of validating new projective techniques through predicting, on the basis of other diagnostic results, what patients will do on the new instrument. Checking these predictions, altering hypotheses, and making predictions again will permit the new technique gradually to define its own validity.

Sarason (168) specifies the variables which constitute the testing situation and argues that the interaction of these variables is typically ignored in making predictions from test results. It is usually assumed, for example, that the same needs are involved in the testing situation as in the patient's life situation or in the therapeutic situation to which the patient may go after diagnostic investigation. Sarason questions this assumption, and considers certain discrepancies in published research the consequence of a failure to evaluate the subject's "internal responses" to stimulus-configurations.

The problem of an apparent shift in patterns of need-satisfaction from one stimulus-situation to another becomes most pointed, of course, when predictions are made on the basis of projective data. This matter receives careful consideration in a paper by Korner (119), directed toward the rationale underlying projective techniques. Korner begins with her own finding (118), supported in less formal but still convincing ways by any clinician dealing with young children, that hostility shown in a projective play situation is not related to the amount of hostility expressed in "reality" situations. Certainly there are intervening variables between the subject's needs and

the adaptations he makes to them in everyday life situations; consequently, one should not expect reality behavior to correspond perfectly with fantasy or with pathology. Korner concludes by questioning whether the prediction of "reality" behavior is a legitimate criterion for the validity of projective techniques. The reviewer would conclude by questioning whether behavior in a play interview is any less "real" than behavior on the school playground. In either case, the choice of predictive criteria for projective methods becomes crucial.

Theoretical orientation.—Presumably every application of an old diagnostic technique, as well as every development of a new one, is dictated by some hypothesis regarding human behavior. Typically, these hypotheses are implicit rather than explicit; the diagnostician does not share with others and may not even specify to himself the view of personality organization which directs his study. Consequently, most publications in the field of psychodiagnostics lack explicit statements of theoretical orientation. This year, however, there are signs that clinicians are becoming self-conscious about their theoretical affiliations. As might be expected, the stated theoretical preferences cluster about the centers of greatest activity and controversy in the general area of personality: psychoanalysis, field theory, and the interrelations between personality and perception.

The 1949–1950 University of Texas Clinical Psychology Symposium on perception, now published in book form under the editorship of Blake & Ramsey (17), includes much material which is useful in relating diagnostic methods to the larger field of perception and personality. The papers of Bruner (25), Hilgard (88), Miller (135), Cameron (29), Klein (112), Dennis (44), and Frenkel-Brunswik (64) all deal with principles having direct relevance to the problem of psychodiagnosis. Occasionally, the relationship becomes explicit, as in the use made of Rorschach results by Klein, Dennis, and Frenkel-Brunswik. Klein, for example, presents hitherto unpublished data indicating that subjects who are "form-bound" on the Rorschach also have difficulty in seeing apparent movement in the phi phenomenon test situation (113). Here a general attitude of reluctance to take an "as if" attitude (intolerance of ambiguity, in Frenkel-Brunswik's terms) seemed to determine the selective perceptions which constituted the Rorschach profile. In a more general way, many contributions to the symposium emphasized the importance of the subject's training, set, or pervasive attitude in determining selectively his perceptions. This, of course, is the clinician's familiar "projective hypothesis."

The close tie-up between this perceptual hypothesis and contemporary research in psychodiagnostics is seen in the sizable number of studies in which set or attitude is experimentally varied. Hutt, Gibby, Milton & Pottharst (66, 103), for example, obtained two Rorschach records from each of 92 college students within a two-week period. Twenty-seven different examiners participated. The first test was administered under standard conditions, the second under one of four different conditions: (*a*) control; (*b*)

intellective (the subject was asked to attend to segmental areas of the blot); (*c*) fantasy (the subject was instructed to attend to human movement); (*d*) conflicting instructions (the subject was asked to use color, give only good form, and see people).

Partial analysis of the results indicates variability in the relative reliabilities of the various scoring categories. It indicates also that the "intellectual" variables are not stable under different experimental sets and that they are not equally unstable. Thus W and (D+Dd) appear least stable, and F+ and A most stable. M increases sizably under condition (*c*). An interesting byproduct of this study is the finding that the stability of the "intellectual" variables is inversely related to the adaptability of the subject. Perhaps sensitivity to varied experimental sets is one indication of a capacity to shift flexibly—a sign of a mature, healthy personality.

A similar approach characterizes Lord's study (122), in which 36 male college students were given three Rorschachs at four- to six-week intervals. Three different atmospheres—negative, neutral, and positive—were developed in the various sessions. Here experimenter-differences constituted the most pervasive influence in the study, although a majority of the scoring categories varied as the testing atmosphere changed. On positive administration, there was a steady rise in mean ΣC and an increase in M and FM. Somewhat contrary to expectation, K, FK, and C′ had higher mean frequencies on positive than on negative administrations, almost as if positively loaded situations were more threatening (or challenging) than negatively loaded ones. When the M:ΣC ratio was used as an indicator of stability, then the more "stable" personality records showed less shift in response to changing atmosphere than did the "unstable" records. This might be considered contradictory to the findings of Hutt, *et al.*, although the two studies differ so markedly in the sort of set which is induced that it is impossible to compare them directly.

Four other studies show the effect of set upon selective perception in the diagnostic situation. Meltzoff (132) administered 90 incomplete sentences to 120 male undergraduates under four conditions: (*a*) the subject was instructed to respond as if he were extremely well-adjusted; (*b*) he was to act as if he were emotionally disturbed; (*c*) the subject's self-esteem was threatened; and (*d*) his anonymity was guaranteed. It is clear from the results that subjects can manipulate their responses to this test to create a convincing impression of good or poor adjustment. When their self-esteem is threatened, the subjects respond in the same way as those who are acting well-adjusted, although they take longer and are more evasive in their answers. In contrast, subjects working anonymously respond most rapidly and give more maladjusted and fewer well-adjusted and evasive responses than the threatened subjects.

Using a similar approach, Carp & Shavzin (32) obtained from each of 20 college students two Rorschach records, three weeks apart, under instruc-

tions to give "good" and "bad" impressions of themselves. Although no significant group differences emerge from this study, there are wide and significant differences in subjects' abilities to shift their records with shifting sets. McReynolds (128) assumed that perception differs from one deviant group to another and proceeded to study the differential perception of ambiguous stimuli by subjects representative of different abnormal groups. After administering the Rorschach in the usual way, he asked each subject whether he could "see" in specified areas of the blots 50 different concepts; the subject merely answered "yes "or "no." With the responses of 54 normal subjects as a reference point, McReynolds analyzed the frequencies of responses of five groups suffering from behavior disorder and one group of feebleminded subjects. The over-all differences in means of each of the variables for the various samples are all statistically significant, from which the author concludes that subjects with different personality structures perceive the Rorschach blots differently.

The final study in this group utilizes personality ratings rather than test performance. Kelley (106) gave college students preinformation about the "warmth" and "coldness" of a new instructor and then obtained from them free descriptions and 15 personality ratings of the instructor at the end of the class hour. The students' perceptions of their instructor reflected the influence of the preinformation, with unfavorable information leading to unfavorable perception and to the restriction of social interaction.

More ambitious than these appropriations by diagnosticians of parts of contemporary perceptual theory is the attempt of Abt & Bellak to provide a theoretical foundation for "projective psychology" (2). Two introductory chapters in their recent book concern the construction of a theoretical system to support projective techniques. As Rotter points out (164), one may question the need for a special system for particular techniques, but the attempt is still a provocative one. Bellak's theoretical contribution (10) is predominantly psychoanalytic in orientation, although he extends the conventional meaning of the term projection and proposes to substitute for it the word "apperception." Apperception in this sense includes projection (both inverted and simple), sensitization, and externalization. Projective techniques invite apperceptive distortion and illustrate the structuring influence of the memory of past apperceptions upon present apperceptions.

Abt's chapter (1) on the other hand, attempts to reconcile psychoanalytic and Gestalt personality theory and to employ concepts from both in dealing with projective psychology. The appropriate concepts, as employed by Abt, are highly general. Thus personality is a process, influenced by person-environment interaction and by personal need; the projective psychologist is interested in "personality as a whole." Selective perception finds its way into this formulation, both through the route of Gestalt perceptual theory and through such internal frames of reference as Rogers' "self-concept." Abt conceives of projective psychology as a "psychology of protest" which must

be prepared to dispense with behavioristic approaches and to replace them with a "functional" study of the individual. As first attempts to systematize the thinking which has produced projective psychodiagnostic techniques, these chapters are stimulating, even though their immediate relationship to investigative work in psychodiagnosis is not yet clear.

In a more restricted and consequently more precise fashion, Bühler (27) demonstrates how contemporary psychoanalytic theory supports current diagnostic methods. She begins with French's concept of integration, to which the notion of integrative capacity is central. She then uses the Rorschach as an example of an integrative task which tests and measures the subject's integrative capacity. Her Basic Rorschach Score, the algebraic sum of certain weighted Rorschach signs, varies along a dimension which appears similar to personality integration in French's sense. Basic Rorschach Scores from 500 patients can be subdivided into four levels ranging from I, the "adequate" person, to IV, the overthrow of ego organization. This paper illustrates the rare case in which the details of a comprehensive personality theory can be related to the detailed scoring of a complex projective technique.

The hazards to which the diagnostician studying psychoanalytic theory may expose himself are illustrated in an exchange of papers on Blum's now well-known "Blacky Test" (19). Seward (170) carefully reanalyzes the original reported results and reduces the proportion of findings in agreement with psychoanalytic theory from 22 out of 23 to 16 out of 18. His criticisms are directed more toward logical inconsistencies in the psychoanalytic theory underlying the test than toward the test as a diagnostic instrument, however. Blum's reply (20) to Seward reaffirms his conviction, shared by increasing numbers of clinicians, that psychoanalytic theory, whatever its ambiguities, does prove under certain conditions to be testable.

Two further papers may conclude our review of theory-oriented diagnostic studies. In his contribution to the American Psychological Association symposium, Zubin (195) presents a scheme for classifying mental life into four layers and relates certain diagnostic techniques to each level. The first layer, including material which is inwardly accessible and outwardly reportable, is open to measurement through tests and questionnaires. The second level (inwardly accessible but outwardly not reportable) and the third level (inwardly inaccessible but outwardly apparent) can be tapped by projective methods. The fourth level comprises material which is neither inwardly accessible nor outwardly reportable and is not available to investigation through present-day diagnostic techniques, short of psychoanalysis or hypnosis. The other paper, by Wittenborn & Eron (192), is noteworthy as one of the few attempts to handle projective material within the framework of the Yale University drive theory. The authors employ the drive-evocation—drive-reduction paradigm to predict, with relative success, how TAT responses will correlate with one another.

Trends in Research Methodology

For years it has been fashionable for the clinician to deplore the lack of experimental and statistical techniques appropriate to his subject matter. There was a time when his complaint was thoroughly justified. Methods of appraising group trends and differences obscured the clinician's chief concern—his individual patient. Instrumentation designed to elicit simple, clear-out, overt responses seemed to be unsuited to a study of fantasies, dreams, and other relatively covert but highly significant behavior. Measurement directed toward single dimensions of response violated the diagnostician's faith in the importance of "patterns" of simultaneous interaction among many variables. The situation was distressing enough to persuade many research-oriented clinicians to await, as patiently as possible, the development of new research methods. The only alternative seemed to be the design of studies which, though precise and well-controlled, were trivial or irrelevant.

There are signs that this comfortable defense is beginning to break. One turning point was certainly the symposium on statistics for the clinician, published last year under the editorship of Zubin (196). This symposium did more than present new analytic techniques appropriate to the research needs of the diagnostician. It made clear that the complex problems of the clinician were going to have complex and time-consuming solutions. Just as there are no get-rich-quick schemes of diagnosing individual personality, so also there are no foolproof short cuts to significant research on psychodiagnosis.

Another development leading toward meaningful research has come from a changing attitude among practicing clinicians themselves. Obviously, a diagnostic instrument is constructed for the practical purpose of diagnosis. But the very properties which make it a useful diagnostic instrument may render it practically immune to systematic research investigation. An item, e.g, a colored card on the Rorschach, is included because, on both pragmatic and theoretical grounds, it should elicit a reaction which tells us something significant about the patient. But when we attempt to specify what exactly leads to this reaction, we find ourselves at once in a confusing situation. The card is colored, to be sure. It is also different from the other cards in outline or in relative amount of white space. It occupies a particular ordinal position in the series and is preceded and followed by particular cards, to which also the patient has given his own idiosyncratic response. So long as this pattern of standard stimulus and administration is maintained, we cannot specify which configuration of properties, among all those which characterize the card, is most involved in the patient's reaction. This situation is not, of course, peculiar to the Rorschach. The same may be said for almost any item in almost any diagnostic instrument.

A great many practicing clinicians have felt that the standard properties of diagnostic techniques should be inviolable. Any research attack on these

instruments should preserve the items, the administration, and the scoring in the form that they are used clinically. There is much to be said in favor of this viewpoint. If research findings are to be immediately applicable, they must derive from situations closely similar to the ones to which they are applied. The point at issue here, however, is the limitation on meaningful research which this requirement imposes. For many contemporary diagnostic instruments, this restriction guarantees the confounding of important variables, so that clean-cut answers to research problems are impossible. It makes the research method of choice the gross approach of comparing criterion groups to which standard diagnostic techniques have been administered under standard conditions. Perhaps this is one reason for the popularity, noted earlier in this review, of criterion group comparisons in contemporary research in psychodiagnosis.

Because of the recognized limitations of this approach, some psychologists have shifted their attitudes toward the inviolability of diagnostic instruments. The work of Siipola, Kuhns & Taylor (171) on color reactions to ink blots is a good example. The stimulus-materials are simple forms, rather than the complex Rorschach cards, in which color can be manipulated with relative independence. The work of Hunt and his associates (62, 101, 102) on "scatter" is another illustration. Rather than dealing with the complex patterns of Stanford-Binet or Wechsler-Bellevue response, Hunt constructs a Comprehension-Vocabulary-Similarities (CVS) scale designed in part to elicit scatter and then studies the properties of scatter under various conditions. Both investigations yield results (to be discussed later in this review) which have important consequences for Rorschach and intelligence-test interpretation.

It seems clear that increased statistical sophistication and a greater willingness to examine separately the components of standard psychometric instruments characterize this year's trends in research methodology. Some illustrative studies may give the flavor of these trends.

Experimental approaches.—True "experiments," in the sense of the control and systematic manipulation of variables, are few. The Rorschach studies of Siipola, Kuhns & Taylor (171), mentioned earlier, successfully manipulate color as a determinant of the perception of ink blots. The Rorschach investigations on "set," also described earlier, represent similar successful attempts to alter selective perceptions through systematic manipulation of instructions or "atmosphere." To these should be added the study by French (63) of the effect of experimentally induced frustration upon performance on the Rosenzweig Picture-Frustration Study. A class of college students took the P-F test; three weeks later they took an essay examination on course material and obtained grades which were systematically falsified; immediately after receiving their grades they took the P-F test again. Analysis of covariance revealed some changes in score attributable to the induced frustration, although these were so small as to have little practical significance.

Scaling procedures.—Particularly in the case of projective methods, scaling procedures are being devised to quantify and make comparable the varied and complex scores obtained. Zubin has long been associated with this development; he reviews some of his own work, as well as that of others, in his contribution to the American Psychological Association symposium (195). His techniques have been applied by Eron (51) and by Eron, Terry and Callahan (53) to the rating of "emotional tone" in the stories and outcomes of TAT protocols. Zubin believes that, because of the time and effort required, such scaling devices must be considered primarily research tools rather than practical clinical techniques.

Profile analysis.—The question of how to handle simultaneously a number of scores achieved by the same subject remains a difficult one. Two interesting contributions to the solution of this problem have been made this year by Meehl and Hathaway, in connection with the Minnesota Multiphasic Personality Inventory (MMPI). In an article on configural scoring, Meehl (129) presents a particularly lucid statement of the assumptions involved in pattern analysis. His own contribution, however, is at the level of the single test item, rather than the configuration of subtest scores. He begins with the example of two test items to which two clinical groups respond oppositely. Considered separately, these two items do not predict the criterion; taken together, they do. He then derives a "configural validity coefficient" as a function of the algebraic difference between inter-item correlations in two criterion categories. Presumably graphic methods could eventually be employed to select potentially valuable item pairs in instances where single-item analysis may give negative results.

The other contribution to MMPI profile scoring is not new this year, but achieves its most extensive application in a current publication. In 1947 Hathaway (85) proposed a system for coding MMPI profiles, which forms the basis of the monumental 1951 *Atlas for the Clinical Use of the MMPI* (86). The coding of any MMPI profile involves (*a*) arranging, in order of descending score, the numbers of all MMPI subtests above a T-score of 54 and (*b*) arranging, in order of ascending magnitude, the numbers of all subtests below a T-score of 46. The L, F, and K scores follow. Essentially this procedure yields a sorting of MMPI T-scores into "high-point" and "low-point" groups for any given subject. The atlas includes 968 brief case histories, arranged according to similarity of profile code. A careful reading of these groups of histories should provide the clinician with sufficient "feeling" for a particular code to enable him to use it as a short-cut in evaluating the next patient whose MMPI profile he sees.

Wechsler-Bellevue subtest analysis, as we shall see later, is still a popular vehicle for diagnosticians concerned with profiles. Two investigations in this area employ novel analytic techniques. Harper (79) uses Fisher's discriminant function in comparing the raw Wechsler-Bellevue subtest scores of 245 schizophrenics and 237 matched normals. The relative contribution

of each subtest to the total differentiation between the groups is expressed in terms of beta weights; and the scores of the two groups are predicted by means of the multiple regression equation. Harper suggests the application of this equation to the raw subtest scores of any test record to determine the likelihood of its being "schizophrenic" or "normal." His own use of the equation in diagnosing a new group of 56 schizophrenic patients resulted in 68 per cent correct hits. Kogan (116) computed Wechsler subtest deviation scores for groups of schizophrenic and neurotic patients. He then compared the profile of each subject with that of every other subject, subtest by subtest, noting the distance between each pair of subtest scores. The measure of "agreement" between patterns was the sum of the differences between pairs of scores. Kogan found that pairs of subjects having close agreement in total Wechsler subtest pattern were no more likely to represent the same diagnostic category than were subjects whose profiles showed little agreement.

Factor analysis.—A few diagnostic studies this year utilize factor analytic techniques. Wittenborn's factor analysis of symptoms of behavior disorder has already been mentioned (191). Wittenborn has also published a factor analysis of 21 Rorschach scoring categories (189), based on the responses of 91 college undergraduates. This study begins with four hypotheses: (*a*) that the factorial pattern for M is different from that of color responses; (*b*) that W, CF, C, c, and diffusion categories are intercorrelated and differentiated from F, M, and D; (*c*) that there is a general factor of "productivity," which (*d*) enters differently into the various scoring categories. Application of the method of centroid analysis yields four factors whose composition supports these four hypotheses, and which indicate in addition that FC is more similar to M than to CF and C. It seems clear that, as far as this study is concerned, conventional Rorschach scoring puts together categories (color, texture, and diffusion, for example) which have dissimilar factorial compositions.

Proceeding from a different hypothesis, Hughes (97) factor analyzed 22 Rorschach "signs" of organic, neurotic, and schizophrenic pathology, using responses from 100 patients representing these three diagnostic categories. He obtained, in addition to a residual and error term, seven orthogonal factors, provisionally identified as (*a*) organic pathology, (*b*) low R, (*c*) loose conceptual thinking, (*d*) color awareness and use, (*e*) popular response, (*f*) psychoneurosis, and (*g*) confused succession. A weighted factor key, based on these results, was then applied to 118 additional cases. Although Hughes obtained a number of significant differences in factor scores among his groups, he recommends only the first factor (organic pathology) for diagnostic use. This weighted factor score identified 82 per cent of his second group of organic cases and falsely identified only 1 per cent of his nonorganic subjects.

Wheeler, Little & Lehner (185) factor analyzed the raw MMPI scores of 110 male neuropsychiatric patients and 112 male college students. The normal sample yielded two major group factors: (*a*) "concern with self,"

with high loadings on the Sc and Pt scales and high negative correlations with the K scale; and (*b*) "neurotic adjustment," with high Hy loadings and also high K loadings. The patient sample also yielded two major factors: (*a*) heavy Sc and Pt loadings, as in the normal sample; and (*b*) heavy "neurotic" (Hy, Hs, D) loadings, but no significant K loading, in contrast to the normals. It appears, from the K loadings, that the self-defensive techniques which are intact in the normal sample are weakened or absent among the patients. The position of the Pa scale is different in the two groups, occurring in combination with the "psychotic" scales for the patient sample, but with the "neurotic" scales in the normal group. These findings suggest that the MMPI, at least in its factorial composition, may be differentiating neurotic from psychotic responses more precisely than it is differentiating clinical syndromes.

One application of the P-technique variety of factor analysis has appeared this year in a paper by Cattell & Luborsky (34). Daily responses of an ulcer patient in therapy to a battery of psychological and physiological tests over a long period of time are factor analyzed. In addition to the familiar personality factors, this analysis produces a factor related to the course of therapy and offers evidence for the commonly postulated negative relationship between overt anxiety and gastrointestinal complaints. Although the P-technique is too demanding and time-consuming for routine use, it remains a highly provocative method for clinical research.

Other single-case presentations, employing less rigorous methods than this, contribute to this year's fund of published diagnostic information. The cases now appearing regularly in the *Journal of Projective Techniques* provide excellent examples of the combination of diverse diagnostic results into a coherent picture of one subject. The cases of a 10-year-old boy, presented by Schwartz (169), of a husband and wife in marital discord, analyzed by Buck (26), and of a severely neurotic, possibly latently schizophrenic patient, presented by Mercer & Wright (133), are illustrations of this trend.

Genuinely "experimental" approaches, increased precision in scaling procedures, and developments in profile and factor analysis define only the most clear-cut trends in research methodology in psychodiagnostics. It seems to this reviewer that research methods this year are in general more appropriate and more sophisticated than in the past. A great many investigators are taking advantage of the economies of analysis of variance and covariance. Most writers faced with the inevitable condition of a limited number of subjects are using the appropriate small-sample techniques. And, although it may appear to many readers unworthy of note, almost no studies found a publication outlet this year if they lacked a control group.

Applications of Projective Techniques

Projective techniques still occupy the favored position in published research, and comparisons of groups representing different psychiatric cate-

gories still constitute the most common approach. Bellak reviews some of these studies in his contribution to the 1950 volume on progress in neurology and psychiatry (9). Over the past three years, however, an increasing number of papers have dealt with the rationale behind projective testing, with refinements of administration and scoring, with normative problems, and with the basic properties of available scales. The epidemic of new, untried projective devices seems to be subsiding.

The book on projective psychology edited by Abt & Bellak (2) presents, in addition to the theoretical contributions mentioned earlier, summaries of the administration, scoring, and results of a number of different projective techniques. The chapters, each written by a different contributor, cover the Rorschach, TAT, Szondi, Bender-Gestalt, sentence completion, and mosaic tests, as well as figure drawing and finger painting. Two final chapters in this book deal with projective tests in nonclinical areas. Brower & Weider (23) review the applications of projective techniques in business and industrial settings. After discussing a few simple projective approaches which may be helpful in determining the potentialities of job behavior, they suggest the following battery of techniques suitable for industrial use: Cornell Word Form, for screening purposes; Tomkins-Horn Picture Arrangement Test, sentence completion and draw-a-person tests; mirror drawing, to measure reactions to stress; and, in special cases only, the Rorschach and TAT.

Proshansky (148) describes the use of projective methods in action research, particularly in community situations where concealment of the social issues being investigated is necessary in order to obtain valid results. In this connection, Proshansky utilizes a definition of projective techniques which emphasizes what the clinician does with the data he obtains from his subject, rather than the stimulus-situation which elicits his subject's reactions. ". . . the basic requirement of the projective technique is that the data be interpreted projectively—that is, according to some pre-established theory. . . " [(148), p. 468]. From this, it follows that any investigative method may be utilized projectively, provided only that the results are interpreted according to some prior conceptualization of what the subject's responses mean. A separate article by Meltzer (131) demonstrates this approach in connection with interview procedures. Using as examples cases selected from his files, Meltzer shows how responses to interview questions reveal distortions which may be interpreted projectively.

Most psychologists, including the authors of the foregoing papers, would agree that there are dangers in the projective interpretation of subjects' responses. Carp (33) makes this point neatly on the basis of experimental evidence. She studied the variable "constriction" as measured in the crayon drawings, play constructions, and Rorschach protocols of young children. The measures of constriction were the number of colors and the area used in the drawings, the number of objects and the area used in the play constructions, the ratings by psychologists on a seven-point constriction scale,

and the Rorschach F percentage. Teachers' ratings on personality traits and results of a sociometric analysis of the classes were also available. Carp found no meaningful agreement among the tests on any of the constriction measures, and no meaningful agreement among projective test results, teachers' ratings, and sociometric information. The psychologists, however, showed close and significant agreement with each other in their ratings. These results, as Carp points out, do more than question the consistency with which constriction is defined and expressed from one projective situation to another. They cast doubt as well on the wisdom of accepting clinician-agreement in interpretation as evidence of validity.

Rorschach studies.—The greatest number of publications in the field of projective techniques concern the Rorschach method of personality diagnosis. The Abt and Bellak volume (2) contains three chapters on general Rorschach problems. Perhaps the most novel of these is Lindner's scheme for content analysis of Rorschach protocols (121). On the basis of his experience, Lindner has isolated 43 different content responses, each of which seems to relate to, or to occur frequently in, particular diagnostic categories. He describes these responses and their meanings in highly specific terms. Thus, for example, a W response of "eagle," "bird," or "bat," with movement of the wings, on Card I is a useful clue in separating paranoid schizophrenic patients from obsessive patients with paranoid elements. Lindner notes that his approach to content analysis requires severe statistical and empirical test before it can be accepted fully by Rorschach workers.

In her contribution to the Abt & Ballak volume, Munroe (139) describes her inspection technique for the Rorschach protocol. She includes hitherto unpublished material on the validity and reliability of the inspection Rorschach adjustment measure as well as detailed instructions for the use of the inspection check list. Harrower's chapter (82) deals with the group Rorschach and the multiple-choice Rorschach. A brief description of the techniques is followed by a review of some results of their application in industrial, hospital, educational, and military settings. The negative findings, which largely characterized these procedures in the armed forces, are included here and explained partly in terms of different ingredients of "adjustment" under peacetime civilian and wartime military conditions. Not included in the chapter is a later paper by Hire (89), which reports results of the free writing of responses and inquiry by a group of 50 college students. The study indicates that, as compared with individual administration, this group technique invites high general productivity and high frequency of the various scoring categories.

The remaining Rorschach publications deal with aspects of three general topics: (*a*) normative data, (*b*) stimulus-properties of the cards, and (*c*) applications to diagnostic groups. Each of these will be discussed.

(*a*) Normative data: Three papers present partial results of the normative studies in progress under Beck's direction. Beck, Rabin, Thiesen, Molish

& Thetford (8) obtained Rorschach records from 157 employees of a mail-order house, varying in age and occupational level. They report detailed results from this sample on all determinants. Thetford, Molish & Beck (174) and Rabin & Beck (151) analyzed the Rorschach protocols in groups of normal school children, ranging in age from 6 to 17½ years. Breakdown of the sample by age reveals some developmental trends in Rorschach response. Thus F+, P, and R seem to increase with age, while W, D, Dd, and M remain relatively constant. These investigators find a typical increase in personalized fantasy living and a typical increase in constriction and rigidity, in prepuberty and early adolescence.

A study of Rorschach responses in preschool children by McLeod (126) supports in most particulars the earlier work of Ford (58) on the same topic. McLeod used protocols from 60 children, 20 each at ages four, five, and six, and scored them according to Klopfer's system, to which he added a qualitative classification of location scores. He finds that, with increasing age over this three-year range, there is a decrease in arbitrary, confabulatory, perseverative, and vague responses and an increase in the objectivity, elaboration, and differentiation of perceptions.

Roe's studies of the personality organization of physical scientists appeared this year in two papers. In the first publication (158) she reports the results of group Rorschach testing of 65 members of the faculties in physics and physical chemistry of six large schools which grant doctorates. As contrasted with the results she obtained for biologists (157), her analysis of physical scientists' protocols yields relatively few significant findings. The outstanding characteristic of the physical science group as a whole is the high incidence of color and shading shock. This group tends also to see unusual details, to give white space and inanimate movement responses, and to reveal strong, easily mobilized basic anxiety reactions. These scientists' concern with three-dimensional space and with inanimate movement may well have vocational importance, although these concerns seem to be correlated, in total personality organization, with cautious affectivity and depressive tendencies.

Roe's second publication (159) presents the detailed results of the individual study of nine theoretical physicists, nine experimental physicists, and four physical chemists, all ranked as distinguished contributors to their fields. Information concerning family background, early history, professional history, psychosocial development, religion, and recreation is provided, along with results of a verbal-spatial test, selected TAT cards, and the individual Rorschach. Munroe's inspection technique was employed for most of the Rorschach analyses.

The theorists in this sample seem to divide into two groups on the Rorschach. One group gives a great many responses, a loose or confused succession, and a high percentage of responses found in unusual areas; it utilizes a wide range of determinants and content categories. The other group of

theorists gives limited protocols, many whole responses, and frequently poor form responses; it uses few unusual areas. Additional data indicate that the first group is adequately controlled and relatively mature socially, while the second group has protected itself by means of techniques of withdrawal. The experimentalists in Roe's sample, on the other hand, show wide variation in most scoring categories, although they seem as a group to be somewhat lacking in adequate social techniques. The diversity in Rorschach performance among these distinguished physical scientists constitutes another warning against the uncritical acceptance of Rorschach results as accurate pictures of personality organization. It is clear from Roe's findings that a man of unusually high scientific achievement can produce a Rorschach protocol from which signs of capacity for high achievement are absent.

Two other papers concern the Rorschach performance of superior adults. Holtzman (91, 92) utilized self-ratings and others' ratings of 46 university men in the same living group as criteria against which to validate Rorschach signs of shyness, gregariousness, and impulsiveness. A sufficient number of his results are negative to call into question the conventional Rorschach signs of these personality traits. The delineation of *Erlebnistyp* from the ratio (Fc+c+C′):(FC+CF+C), for example, is not significantly correlated with shyness or gregariousness, nor is absence of color from the Rorschach record. The correlation between impulsiveness, as measured by eight Rorschach signs, and behavior ratings of impulsiveness is .49. The most important single indicator of impulsiveness seems to be spontaneous remarks provoked by color. Within the limitations of overt behavior as a criterion for the validity of projective techniques, a point discussed earlier in this review, these studies raise highly significant questions regarding certain widely accepted Rorschach signs.

Neff & Lidz (140) analyzed the individual Rorschach protocols of 100 Army Medical Corps personnel, selected to represent the normal distribution of Army General Classification Test (AGCT) scores. In this sample, only men with "superior" intelligence achieved, in their Rorschach performance, the standards usually considered typical of normal persons. Men falling into "average" and "inferior" groups with respect to AGCT score gave Rorschach records which would ordinarily be considered neurotic.

Three papers attend to selected aspects of Rorschach scoring rather than to the over-all performance of normative groups. Thompson (176), using group administration and the Munroe inspection technique with two samples of college students, demonstrates a significant positive relationship between "popular" responses and the length of the Rorschach record. Sanderson (167) attempts to set up norms for "shock," in terms of initial reaction time to each card. It is clear from his results, based on the responses of 50 adults coming to a vocational guidance agency, that each card has its own reaction time, with the longest times occurring on the cards most frequently rejected. Kimball (108) presents a historical summary of form-level appraisal

on the Rorschach, and then studies the reliability of evaluations of form level made by appraisers with varying degrees of Rorschach experience (109). Her findings indicate wide discrepancies among experienced as well as inexperienced Rorschach workers in dealing with this important scoring dimension.

Ranzoni, Grant & Ives (153) combine a normative study with an investigation of the stimulus-properties of the Rorschach cards. The responses of 194 adolescents, ranging in age from 11 to 16 years, are tabulated card by card, and scoring categories compared both within each card and among the 10 cards. This analysis demonstrates the importance of "card pull" in determining the relative frequencies of scoring categories for the various cards. The first six cards, for example, pull a high proportion of W responses in this group, but the last four cards are less likely to elicit whole responses. The cards as a group show a high over-all F pull, but a weak color pull. Results such as these call into question some Rorschach interpretations found in the literature; certain presumed "signs" of pathology may be nothing more than the responses to be expected from these particular stimulus-cards.

(*b*) Stimulus-properties of the Rorschach cards: The effect of color as a determiner of Rorschach responses remains a popular topic of investigation this year. The experimental study of Siipola, Kuhns & Taylor (171), mentioned earlier, is a good example. These workers presented simple stimulus-forms twice, at a monthly interval, to each of two groups of college women. The experimental group saw chromatic cards followed by achromatic ones; the control group saw the same achromatic cards on the two occasions. The experimental group failed to recognize the majority of the critical achromatic blots, while the control group achieved significantly high recognition scores on the second testing. Judging from these results, hue decreases recognition; consequently, one might use a recognition score as a measure of the extent to which subjects are color-influenced. Color also increases the number of different responses given by the experimental group and decreases the number of identical responses given. Since the control group also gives a high proportion of different responses to the same blots, however, it is not possible to specify which of the different responses in the experimental group are determined by color. In this study, there are no significant group differences in reaction time which may be attributed to color.

Similar results emerge from two other studies of color. Dubrovner, Von Lackum & Jost (47) presented one of two sets of cards—one standard and one achromatic facsimile—to two groups of student nurses and retested their subjects with the other set some days later. They found no evidence that color affected response productivity or reaction time and no support for using the percentage of responses made to Cards 8, 9, and 10 as an index of emotionality. The latter point is affirmed in the study of Perlman (146), where test-retest procedures with standard and achromatic cards were also

employed. Perlman found that the achromatic cards elicited about the same range in percentage of responses to the last three cards as expected with the colored cards; he concludes that individual differences in this percentage cannot be attributed to the color properties of the cards.

In one part of a larger study on the stimulus-properties of the Rorschach blots, Meer & Singer (130) attempted to test the assumption that Card IV represents the "father" and Card VII the "mother." Fifty male college students, asked to select two of the ten cards, one "mother" and one "father," gave significantly differential choice reactions to the cards. Cards II and IV tended to be chosen as "fathers" and Cards VII and X as "mothers."

(*c*) Applications to diagnostic groups: Research on the diagnostic accuracy of the Rorschach takes two general lines: the evaluation of over-all performance, ordinarily in terms of "signs" or scales, and the comparison of groups by means of scoring categories considered separately. An example of the former approach is the study of Morris & Nicholas (136), which compares the Rorschach responses of 46 children and adolescents diagnosed as primary behavior disorder with the responses of their parents. The authors use a 6-point rating scale on each of 20 variables, later combined to give an "over-all pathology" score. There is significant homogeneity among all three members of the same family in most of the scoring variables. Although the children differ significantly from their parents in over-all pathology, there are signs of pathology in the parents' records also. Morris & Nicholas suggest that pathology in these children may be the consequence of a cumulative reinforcement effect, in the children's personality development, of the parents' pathological personality traits.

Fein (55) examined the Rorschach records of 43 male college students—9 homosexuals, 10 neurotics, and 24 normals—for signs of homosexuality. Although her analysis yields some homosexual indicators, it calls others into question. Paranoid responses and sex confusion, for example, are more characteristic of general insecurity and uncertainty than of homosexuality. Hamlin, Albee & Leland (76) checked Muench's Rorschach signs of adjustment (137) in normal, maladjusted, and neuropsychiatric subjects and found no significant differences among individuals at different levels of adjustment. Correlations between Rorschach movement responses and Wechsler-Bellevue IQ were calculated by Tucker (177), for a group of male neurotic veterans. Most of the correlations obtained were significantly different from zero, but not high enough for predictive purposes.

Somewhat more encouraging results were obtained by Salfield (166), who studied 10 signs of neurosis in the Rorschach records of 42 children. He considers the presence of more than five or six signs indicative of neurosis and finds that these signs provide a better prognosis for therapeutic outcome than do three clinical interviews. Kates (105) analyzed the Rorschach records of 25 patients diagnosed anxiety disorder and 25 diagnosed obsessive-compulsive disorder, utilizing the Munroe inspection technique. The

anxiety patients were characterized by more signs of maladjustment and greater helplessness than were the compulsive patients. This, of course, is consistent with the hypothesis that the patient suffering from compulsive disorder handles by means of his symptoms the sort of anxieties which the patient diagnosed anxiety disorder is helpless to control.

Wittenborn (190) factor analyzed the most common scores in the Klopfer system that were obtained from a group of psychiatric patients. He identifies four factors, (*a*) W, c, C, and vista responses; (*b*) M, FM, FC, and d; (*c*) F, D, and R; and (*d*) S and O. It is clear that there are some differences in the factor patterns shown by this patient group, as compared with the results obtained from college students in Wittenborn's paper (189) cited earlier.

The separate-category approach to psychiatric diagnosis is illustrated by the study of Blake & Wilson on depressive tendencies (18). These investigators postulate that, because the Rorschach represents a threat, the depressive person will give defensive and avoidance reactions, as indicated by high F scores and low M and FM scores. Seventy-four male subjects, dichotomized with respect to MMPI Depression score, provided Rorschach responses; for purposes of this analysis, only the first response to each card was used. Results tend to verify the predictions. As compared with the controls, subjects with high D scores give more unelaborated F responses, more Fc, less M, and less FM. Fisher (56) found the Rorschach records of 20 female conversion-hysteria patients in general constricted and limited, with relatively little M, FC, and FY, but without as much impulsive C as the literature would suggest. Examination of the records in the light of clinical symptomatology shows that constricted records accompany circumscribed, limited symptoms, while loose records are related to more extensive and disabling symptoms. Perhaps lack of constriction in the Rorschach record of an hysterical patient indicates failure to work out restricted hysterical symptoms; consequently, more regressive techniques appear.

The assumed unpredictability of behavior of chronic schizophrenic patients is the subject of a Rorschach investigation by Holzberg & Wexler (95). They administered the Rorschach twice, three weeks apart, to each of 20 chronic schizophrenic patients and obtained high test-retest correlations and relatively few significant differences in mean scores. From these results they conclude that schizophrenic patients are not unpredictable, at least in Rorschach performance. Osborne & Sanders (143) found Rorschach signs of emotional immaturity, impulsiveness, and unrealistic ambition in a group of 15 male patients suffering from duodenal ulcer. Ellis & Brown (50) succeeded in differentiating declining from improving pulmonary tuberculosis patients on the basis of Rorschach scores.

Thematic Apperception Test.—Although there are fewer papers this year on the TAT than on the Rorschach, the same topics of norms, validity, and diagnostic differentiation recur for both techniques. Bellak (11) provides a comprehensive review of current TAT issues and findings in his contribution

to the Abt & Bellak book (2). He presents here his own method of TAT interpretation, based on 14 different scoring categories, and shares with the reader his experiences concerning the typical themes and conflicts elicited by each picture and the stories characteristic of variously diagnosed groups. He includes as well suggestions for the self-administration of the test and for its use with patients undergoing psychotherapy. After categorizing the productions of 100 male college students on eight TAT cards, Wittenborn (188) employs correlational techniques in testing the implications of certain assumptions involved in the TAT. His correlations suggest that there is no necessary trait significance in the multiple appearance in a given TAT record of superficially similar roles and themes. On the other hand, TAT responses are significantly more correlated with their complementary responses than are unselected responses correlated with one another; this finding is consistent with any conscious-unconscious theory of personality.

A thoroughgoing normative study of the frequencies of themes and outcomes of TAT stories, based on the responses of matched groups of normal, neurotic, and schizophrenic male patients, is reported by Eron (51). Probably the most significant result of this study is its failure to identify themes or conflicts which are peculiar either to the normal or to the patient samples. Such intergroup differences thus obtained are probably more related to the restrictions imposed by hospitalization than to differences in basic personality organization. In addition, the study raises serious doubts concerning the validity of a good many TAT "signs" of pathology commonly accepted by clinicians. The contributions of this investigation to the problem of scaling TAT responses have been noted earlier.

It will be recalled that Roe's study of eminent physical scientists (159) utilized nine selected TAT cards. Roe analyzed the stories under such headings as narration, description, attention to detail, level of interpretation, feeling-tone, personal relationships, and sources of pressure upon central figures. As a group, these scientists produced rich and significant stories. They revealed an independence of personal relationships, with some tendency to keep emotional situations at a distance. Passivity overlying rebelliousness was present, as well as some free aggression, usually referring to the past.

Bills, Leiman & Thomas (15, 16) conducted six individual play therapy interviews with each of eight children, to whom they then administered the TAT and a set of 10 colored animal pictures. The three techniques were in partial agreement with one another, although the authors believe the animal pictures are superior to the TAT in eliciting stories. Bellak (12) uses the TAT records of two Nazi leaders, both jailed and anticipating death, to demonstrate that TAT protocols may be relatively independent of immediate situational pressures. In spite of the fact that these two men confronted "equal" and overpowering environmental stresses, they produced TAT records which were quite different from each other.

Finally, three papers deal with alterations in the usual TAT procedure.

Korchin, Mitchell & Meltzoff (117) criticize the assumptions underlying the Thompson revision of the TAT, constructed for use with Negro subjects (175). They point out in particular that the change to Negro figures in the plates may reduce, for Negro subjects, both the ambiguity of the stimuli and the psychological distance of the stimuli from the subject. In both cases, the consequence would be more superficial stories. Riess, Schwartz & Cottingham (156) present quantitative evidence that Negro subjects do not tell significantly longer stories to the Thompson TAT pictures than to the Murray pictures. Eron & Ritter (52) compared the oral responses of 30 college men to individually presented TAT cards with the written responses of another group of 30 college men with whom group administration was used. Although the oral stories were longer and included more alternative themes, the two types of administration yielded practically identical story content.

Szondi test.—In view of its relatively recent introduction to American psychologists, the Szondi test has stimulated an unusually large number of publications this year. Considered as a group, the papers present a serious challenge to the assumptions underlying the test. Take, for example, the study by Klopfer & Borstelmann (114) on associative valences of the Szondi pictures. The procedure is designed to test the assumption that, in making their selections on the test, subjects assign consistent, though unverbalized, meanings to the pictures. Six groups of college students gave their free associations to each of the sets of pictures; judges familiar with the test then categorized the associations according to the Szondi factors. The college student subjects participated also in matching the test factors with personality descriptions. These procedures indicate that most of the test pictures have an associative valence, although the obtained categories correspond with Szondi's designation of the pictures in only about half the cases. There is a clear need to study associative designations other than those to which Szondi's original approach restricts the investigator.

A series of papers by Guertin (71, 72, 73) raises additional questions concerning the rationale of the Szondi test. In one (72), 24 dull-normal patients were subjected to 24-hour test-retest procedures; the experimental group took the test in A-A, B-B order; the control group in A-B, B-A order. There was high picture-for-picture agreement in the choices of the experimental group, but only chance agreement among the control subjects; this finding cannot be easily explained by means of the Szondi need-system theory. In another study, Guertin (71) compared the profiles obtained from 55 hospital employees with 68 profiles selected by means of a table of random numbers and found no significant differences in Szondi factor loadings between the two groups. Guertin (73) also compared the Szondi pictures with photographs of 48 healthy American persons, by way of preference rankings made by college students, and found no differences between the two sets of pictures. Rabin (149) asked a group of psychology students and a group of psychologists to match Szondi pictures with diagnostic labels and obtained

extra-chance accuracy from both groups. He later demonstrated (150) that six weeks' training in abnormal psychology improved the performance of college students in identifying the diagnostic groups represented by the pictures. David (43) introduced a posttesting inquiry period into the Szondi administration and found that paranoid schizophrenic patients, as contrasted with a control group of nurses, "recognized" some of the pictures, thought the persons pictured were mostly Americans, and showed uncertainty as to whether the pictures were identical from trial to trial.

A different approach to the problem of validation characterizes Holt's study (90). He argues that the "genotypic reliability" of the Szondi can best be evaluated by correlating Szondi choices with known variations in need. He tested the same subject 12 times, usually at weekly intervals, with the Szondi, an abbreviated TAT, and the Horn Questionnaire. Changes in self-ratings on the questionnaire from time to time are taken as indices of fluctuation in needs. Intercorrelations of all measures on this subject provide support for some of Deri's assumptions concerning the Szondi test.

The theoretical expectations of Szondi picture-choices from zero through six is provided by Cohen (38), as a basis for χ^2 treatment of observed frequency distributions. Fosberg (59) employs this method in evaluating the responses of 100 normals and 100 psychiatric patients. Although the obtained frequencies represent extra-chance deviations from theoretical expectancy, the results do not consistently distinguish the normal from the psychiatric group, or a paranoid subgroup from either unselected patients or normals. In the same paper, Fosberg demonstrates that 43 judges, asked to categorize the personality types shown in the Szondi pictures, agreed significantly with the Szondi designations. Two additional experiments reported in this article, however, indicate that Szondi factors do not show reliably the occurrence either of recent sex activity or of electrically-induced grand mal seizures. In a more extensive approach to the latter point, Deri (45), in her contribution to the Abt & Bellak volume (2), compared the results of Szondi test performance pre- and postelectroshock on two groups of depressed patients with test-retest results in a normal sample. From demonstrated changes in the Szondi s and k factors in the patient groups, she derives support for her hypothesis that electroshock therapy lessens the inhibition of aggression but strengthens repression in the ego. It should be noted, incidentally, that a group administration of the Szondi has been used by Cole & Roberts (39) and that a Szondi IBM form has been developed by David (42).

Other projective techniques.—Woltmann (194) reviews the administration and interpretation of the Bender Visual Motor Gestalt Test in his chapter in the Abt & Bellak volume. This test has been provided with a new scoring system by Pascal, described in a preliminary way in an article (144) and more completely in a recent manual (145). The system utilizes 102 easily noted deviations in the drawings and yields interjudge reliability coefficients as high as .92. Kitay (110) proposes still another scoring system, based on 25

indices of size deviation from the stimulus-figures, which yields equally high reliability coefficients. His scheme permits the calculation of two scores which correspond, on logical grounds, to the Rorschach form and emotional control categories. Hanvik & Andersen (77) find that patients suffering from focal brain lesions are more likely than the controls to rotate the Bender-Gestalt figures, although they do not differ significantly from the controls in the number of figures they can recall.

The Rosenzweig Picture-Frustration Test has been employed in one experimental study by French (63), described earlier in this review. Albee & Goldman (3) attempted to predict overt aggression in hospital patients by means of Picture-Frustration test scores but were unable to do so in any significant fashion. Rosenzweig (162) presents evidence that humorous responses to this test may be safely included in the protocol and scored, and Spache (172) proposes, in scoring the children's form of the P-F test, the differentiation of conflicts with adults from conflicts with children.

The projective interpretation of figure drawing is described by Levy (120) in the Abt & Bellak book, and a rating-scale approach to the evaluation of drawings is suggested by Albee & Hamlin (4). On the basis of 32 figure drawings obtained from paranoid schizophrenic patients, Fisher & Fisher (57) tested two of Machover's assumptions concerning paranoid attitudes and "feeling" of expression and stance. Judges failed to find any sizable number of paranoid indicators in the patients' drawings and could not agree in their evaluations of expression and stance. Holzberg & Wexler (94), on the other hand, found that 27 out of 174 scoring items distinguished, at the .05 confidence level, the female figure drawings of schizophrenic women from those of student nurses. In a preliminary study of figure-drawing in young children, Weider & Noller (182) demonstrated differences in the sex characteristics drawn by children representing different socioeconomic levels. Ochs (142) offers evidence that Goodenough drawings change with changes in the social adjustment of children suffering from primary behavior disorders. An unimproved group of child patients showed a decline in Goodenough IQ, while an improving group increased in IQ. Literature on the production and interpretation of finger painting is reviewed by Kadis (104) in Abt's & Bellak's volume and by Precker (147) in an independent article.

Several articles describe the use of pictures, other than TAT cards, as projective stimuli for the study of children's reactions. Probably the most thoroughly explored of these is the Michigan Picture Test, comprising 20 pictures, which has now been administered to 303 children between the ages of 8 and 14. Hartwell, Hutt, Andrew & Walton (84) describe 12 tentative scoring variables for this test and demonstrate the relationship of some of the scores to total emotional adjustment as indicated by a Teacher Rating Scale. In another publication (7), the same authors evaluate the stimulus properties of 10 of the cards. Broida, Izard & Cruickshank (22) found the Symonds Picture-Story Test useful in distinguishing three groups of crippled

children who differed from one another in the feelings of fear they expressed. Beginning normative data on the Children's Apperception Test (CAT) are presented by Bellak & Bellak (13) in a study of the typical themes given by a sample of 100 children.

Literature on the method of incomplete sentences is reviewed in the chapter which Sacks & Levy (165) wrote for the Abt & Bellak book. These authors give most emphasis to the construction, properties, scoring, and interpretation of the Sacks Sentence Completion Test. In a validation study of the Stein sentence completion test, Rosenberg (161) reports relatively high agreement between attitudes toward parents as determined by case history data and the same attitudes as expressed on the sentence completion test. Wertham's chapter (183) in the Abt & Bellak book provides the fullest description of the Mosaic Test so far published. An independent study of this test by Reiman (155) presents results obtained from five diverse subject-groups and suggests 10 characteristics of mosaics which distinguish male clinic patients from normals. Lowenfeld (123) has reviewed the rationale and administration of her World Technique, and Goldenberg (68) has brought together in summary form most of the published results of the Make-a-Picture-Story (MAPS) test.

Two suggestions for new projective stimuli deserve special mention. Wiltshire (186) employed with children a modified word association test, in which he read a letter series to the subjects, and they responded to each letter with the first word occurring to them. Harrower (83) asked 500 patients to think of the most unpleasant thing they could and then draw it. Results of other projective tests, including the Rorschach, were available for these patients. A fairly wide range of unpleasant concepts—death, pain, illness and the like—was forthcoming. It is of considerable interest, in the light of contemporary events, that the patients who considered war or national aggression most unpleasant were those who showed better personality integration on the Rorschach.

Application of Objective Tests

The use of so-called "objective" measures in diagnosis adds a note of precision, sometimes deceptive and spurious, to the diagnostician's approach to his problems. Many of the studies reported during the past year utilized standard intelligence scales, and a few described the application of paper-and-pencil personality tests. When intelligence or personality-trait scores seem necessary to a useful description of the patient, of course, such techniques are clearly appropriate. Some recent results of objective testing are described in a review by Estes (54).

It is currently much more common, however, for diagnosticians to employ objective measures for purposes other than the ones for which the measures were constructed. The hope seems to be that single subtests, or combinations of subtests, which make up intelligence scales will not only predict criterion

intelligence measures, but will also locate patients in diagnostic categories. There is nothing wrong with such hopes, of course, except that they raise serious questions of validity and of economy. Quite possibly, if the time devoted to analyzing the diagnostic potentialities of intelligence subtests were invested instead in the construction of new diagnostic scales, the result would be measures that are more precise and more appropriate. As it is, when intelligence scales fail, upon controlled quantitative investigation, to yield useful statements concerning personality organization, the clinician falls back upon his knowledge of qualitative indicators. Again, there is nothing wrong with such a procedure, except that at present the communicability of these qualitative indicators leaves much to be desired.

Wechsler-Bellevue Scale:—Historically, these issues have grown more out of applications of the Wechsler-Bellevue intelligence scale then from any other single instrument. Research on this scale during the past year deals with (*a*) general properties of the scale, (*b*) short forms, (*c*) diagnosis by subtest analysis, and (*d*) measurement of deterioration. Literature on these topics published since 1946 is summarized critically in the recent review by Rabin & Guertin (152).

(*a*) General properties of the scale: Because of the widespread use of Wechsler subtests as individual diagnostic measures, the question of subtest reliability assumes great importance. Derner, Aborn & Canter (46) studied these reliabilities, using the test-retest method, with three groups of subjects retested at intervals of one week, four weeks, and six months. They include in their paper a complete review of all published Wechsler-Bellevue reliability studies. It is obvious from these results that the reliabilities differ from one subtest to another. In the Derner, Aborn & Canter study, for example, retest reliabilities range from .62 to .90 on the various subtests. This differential reliability, as has repeatedly been pointed out, undoubtedly contributes to the contradictory and unstable results reported in studies of Wechsler subtest and profile analysis.

A supplementary guide to the administration and scoring of the Wechsler-Bellevue scale is furnished by Kitzinger & Blumberg (111). This manual is helpful in sharpening some of the scoring criteria (based, however, only on group discussions with experienced testers); but it includes diagnostic "rules of thumb" which, in the reviewer's opinion, still lack empirical validation. Gerboth (65) finds, on the basis of the performance of 100 college students, no significant practice effect when Form I of the Wechsler precedes Form II, but he obtains highly significant practice effects, marked in 10 of the 11 subtests, when Form II precedes Form I. It is of some interest that he also finds considerable scatter in this "normal" sample. Practicing psychometricians should note Newton's finding (141) that lowering the pitch of the voice on the final number in the digits-forward subtest enables the subject to recall significantly more digits correctly.

(*b*) Short forms: Last year, McNemar (127) published a highly significant

paper in which he presented a general formula for determining the correlation with total scale score of *n* subtests. Application of this formula enabled him to calculate the correlation between total Wechsler score and the sum of scores on any combination of Wechsler subtests. He then reported the 10 teams of 2, 3, 4, and 5 Wechsler subtests which were "best," in terms of their correlation with total Wechsler score. His approach is important not only in demonstrating the limits of the validities of various Wechsler-Bellevue short forms, but also in indicating that a great many short-form validity coefficients reported in the literature are spuriously high because of the heterogeneity of the samples involved.

Perhaps partly because of the effect of this paper, there are relatively few publications this year which report applications of Wechsler-Bellevue short forms. Gurvitz (75) calculated the correlation of every possible two-test combination with the Wechsler-Bellevue total score, using over a thousand cases of prisoners, vocational guidance clients, and members of Wechsler's standardization group and of Rapaport's patrol. The best combination proved to be Information and Digit-Symbol, which achieved, in this heterogeneous sample, a correlation of .90 with total Wechsler score. A four-test combination of Comprehension, Similarities, Picture Arrangement, and Block Designs correlated .86 with total Wechsler score of institutionalized mental defectives studied by Cotzin & Gallagher (41). Neither of these combinations, incidentally, appears in McNemar's list of the 10 best pairs or the 10 best quartets.

The time pressures under which psychometricians work have stimulated the construction of brief scales in addition to those provided by Wechsler-Bellevue abbreviations. Knott, Cannicoot, Umberger & Bilodeau (115) shortened the Kent battery to five tests and then to four tests. In a sample of psychiatric patients, the five-test combination correlated .86 and the four-test combination .85, with total Wechsler score. Corsini (40) devised a three-minute vocabulary test of 11 groups of six words each, located at successive Stanford-Binet MA levels from 10 to 20. Correlations with the Stanford-Binet range, in different samples, from .83 to .91.

(*c*) Diagnosis by subtest analysis: The popularity of Wechsler-Bellevue subtest analysis as a technique for the diagnosis of behavior disorder continues, in spite of contradictory, confusing, and disappointing results. The probable reasons for the typically inconclusive findings—differential subtest reliability, the unsatisfactory nature of psychiatric diagnosis as a criterion, failure to control age effects, lack of agreement in measures of deviation and scatter—have been stated so often that it seems unnecessary to review them in detail here. Perhaps the basic question is why investigators cling so assiduously to this procedure, instead of starting afresh with the problem of psychiatric diagnosis—if this is a crucial matter—and devising measures appropriate to this criterion. Certainly the persistence of the subtest method testifies to the appeal of short-cut approaches to difficult and complex

problems. Rabin & Guertin evaluate the situation candidly, if somewhat sharply: "It is doubtful that the continued clogging of the periodical literature with testimonials pro and con, based on scanty and uncontrolled evidence, will be of further benefit" [(152) p. 241].

One of the important methodological contributions to this problem has already been noted—Harper's use (79) of multiple regression equations and beta weights to determine the relative contributions of various subtests to the differentiation of diagnostic groups. In a later application of this method, Harper (78) shows that in discriminating schizophrenic subtypes, pattern analysis can be as much as eight times as powerful as predictions based on knowledge of IQ, CA, and education alone. Even here, however, Harper's highest multiple *R* accounted for 41 per cent of the total variance. Another methodological suggestion is that of Holzberg & Deane (93), who propose a measure of "intratest" scatter, to quantify the variability of performance within a single subtest.

Some examples of the more traditional approaches to subtest analysis will indicate the continuing contradictions in this area. From the responses of 100 neurotic and 83 schizophrenic patients, Rogers (160) identified eight subtest interrelationships which distinguished the two groups from one another, but he failed to verify a good many conventional schizophrenic "signs." Gilhooly (67), using a group of 122 psychoneurotic patients, and Warner (180), using a group of 60 anxiety neurotics and 65 controls, also obtained significant differences with certain subtests. However, their results did not agree completely, did not duplicate Rogers', and did not support the "signs" described in the literature. Attempts to diagnose "psychopathic personality" by means of subtest scores meet a similar fate. Gurvitz (74) finds no significant differences among psychopathic prisoners, "normal" prisoners, and randomly selected members of Wechsler's standardization group. Clark & Moore (36) report a significant difference only in arithmetic performance between "psychopathic" and "normal" prisoners. No more encouraging results are obtained by Waldfogel & Guy (179), who fail to find subtest patterns typical of patients diagnosed as manic-depressive manic, manic-depressive depressed, and reactive depression. Combining the three groups yields a pattern characterized by negative vocabulary scatter on performance tests, which may be, to an undetermined degree, a function of age.

Special indices, ratios, and differential weights of subtests still seem promising to many investigators. Andersen (5) reports again his V-score technique for use with patients suffering from focal brain lesions. His results show a greater decline in verbal score in patients with lesions of the dominant hemisphere and a greater loss in performance score in patients with lesions in the nondominant hemisphere. Wheeler & Wilkins (184) applied Hewson ratios (87) to the Wechsler-Bellevue subtest scores of groups of neurotic, psychotic, and normal subjects and succeeded in correctly placing

about 45 per cent of each group. Classification of Wechsler-Bellevue vocabulary responses as descriptive, functional, and categorical by Stacey & Portnoy (173) yielded no significant differentiation among moron, dull-normal, and borderline groups.

Subtest analysis is not confined to the Wechsler-Bellevue scale, of course, even though most studies use that instrument. Hunt's Comprehension-Vocabulary-Similarities scale (CVS) continues to be a promising tool for the measurement of scatter. French & Hunt (62) show that CVS scatter varies directly with intelligence level; the predictive value of CVS scatter diminishes as intelligence level increases. Matarazzo (125) shows that CVS scatter calculated as the ratio of C+S to V gives a better differentiation of paretic patients from controls than does scatter measured as a deviation of C+S from V. Two studies cast doubt on the diagnostic efficiency of the Wechsler Memory Scale. Cohen (37) finds no significant difference in performance on the scale among groups of neurotic, organic, and schizophrenic patients; Howard (96) finds some items which distinguish psychotic paretic patients from psychotic, nonorganic controls, but no significant differences among encephalitic, epileptic, and control patients.

(*d*) Measurement of deterioration: Deterioration measures, whether derived from Wechsler-Bellevue tests or not, depend upon an inference, based on the patient's present performance, of the patient's performance level sometime in the past. Since quantitative statements of past performance are not ordinarily available, the direct validation of deterioration measures is seldom possible. This year, however, two studies use the discrepancy between premorbid and present performance as one criterion for the validation of deterioration tests. Rappaport & Webb (154) located the high school intelligence test records of 10 hospitalized schizophrenic patients, repeated this test, and also administered the Wechsler-Bellevue and the Elgin test-reaction scale. They found a significant loss of IQ points between premorbid and psychotic testing but no combinations of Wechsler-Bellevue subtests which furnished a reliable index to premorbid IQ. Canter (30, 31) obtained two AGCT scores and two Wechsler-Bellevue scores, six months apart, from a control group and a group of male veterans suffering from multiple sclerosis. Correlations between AGCT loss and the Wechsler Deterioration Index, the Shipley-Hartford CQ, the Babcock Efficiency Index, and Hunt-Minnesota scores range from .35 to .46; correlations between Wechsler-Bellevue loss and these measures are considerably lower.

When indirect measures of deterioration are used without reference to this sort of criterion, validity statements are commonly made on the basis of comparisons with other indirect measures or with diagnostic categories. Here a major problem is the allowance which should be made for "normal deterioration" with age; some deterioration measures allow for this, while others do not. What this means for the interpretation of test results is demonstrated in a study by Fox & Birren (60) of 50 men aged 60 to 69. The

Wechsler-Bellevue Deterioration Index, which allows for age changes, diagnoses the group not deteriorated, while the Babcock-Levy, with no age allowance, classifies the group as deteriorated. When a group of hospitalized senile psychotic patients is compared with this control group (21), the Wechsler Deterioration Index does not differentiate the groups, although the Babcock Efficiency Index and Copple's Senescent Decline Formula do. If indirect deterioration measures are to be based on the expected decline with age—a doubtful criterion in the case of many behavior disorders—then the most useful tests are clearly those which maximize that decline.

Objective personality tests:—Paper-and-pencil tests of personality continue to be so overshadowed by projective techniques that they are rarely reported in the literature. The most extensive explorations of these tests are concerned this year with the Minnesota Multiphasic Personality Inventory. The MMPI Atlas, edited by Hathaway & Meehl (86), described earlier, is an excellent and thorough review of MMPI results obtained from thousands of diverse subjects. A start has been made, in the Wheeler, Little & Lehner paper mentioned before (185), toward describing the factorial composition of the inventory. Rearrangement of items in a new Sc scale is reported by Benarick, Guthrie & Snyder (14). The need for some such revision is clear from the results reported by Wauck (181), where less than half of a group of schizophrenic patients achieved Sc T-scores over 70. A similar approach to a new "neuroticism" scale, made up of MMPI items, is employed by Winne (187). Gough (69) summarizes the published material on faking MMPI records and sets cutting scores, based on the F minus K dissimulation index, for the differentiation of authentic from falsified profiles.

The usual attempts to distinguish various diagnostic groups from one another which characterize most contemporary psychodiagnostic research are made with the MMPI. Andersen & Hanvik (6), for example, succeed in differentiating 16 patients with frontal lesions from 27 patients with parietal lesions; the former show MMPI profiles of an hysteroid type, while the latter resemble anxiety neurotics in their test responses. Brown (24) demonstrates that the MMPI profiles of chronic alcoholic patients can be sorted into two groups, one resembling nonalcoholic neurotic patients, and one resembling nonalcoholics who were diagnosed psychopathic personality without psychosis.

One new paper-and-pencil personality test will illustrate the direction of current thinking in this area. Freeman (61) began with the hypothesis that a patient suffering from psychosomatic illness shows a typical constellation of somatic symptoms and personality traits, which constitutes a "psychosomatic syndrome." He then constructed items, including psychosomatic symptoms, personality reactions, and symptoms of established organic pathology. The items were administered to control and psychosomatic groups, and from these results he obtained 71 significantly differentiating items. What is popularly termed "nervousness" is the outstanding feature

of the psychosomatic group. Validation of the scale on independent psychosomatic groups yields a cutoff score exceeded by 80 per cent of the patients with psychosomatic disorder. Freeman takes his results as an indication of the validity of a typical psychosomatic syndrome.

Conclusions

It would be presumptuous to attempt to forecast the direction of future research in psychodiagnosis. If subsequent investigations follow the major trends of the immediate past, however, then, in this reviewer's opinion, the future looks discouraging indeed. One cannot read in detail the contemporary literature in diagnostics without being overwhelmed by the tremendous expenditure of time, money, and labor involved in bringing forth inconclusive or trivial results. It is no accident that, for the past few years, reviewers of this literature have consistently failed to discover in the studies any comprehensive or coherent pattern. Too many investigations are haphazard, isolated, or fragmentary; too many stop where they should begin; too many seem to appear in our journals like rejected orphans, with no meaningful past and no promising future.

It would be less presumptuous, and possibly more optimistic, to indulge in wishful thinking regarding the lines which future research in psychodiagnosis might usefully take. On the basis of this review and others, it appears that clinicians engaged in diagnostic activity have at least three unfulfilled needs:

(*a*) We need critical thinking about the philosophy and goals of diagnosis. This is the question of "diagnosis for what?" The answer to this question, if we believe the typical contemporary study, is "diagnosis for psychiatric classification." To be sure, present-day schemes of categorizing behavior disorders are criticized as overly simple and unreliable, often in the same typical contemporary studies that employ them. And, admittedly, some adherence to traditional usage is necessary if professional persons studying the same patient are to communicate with one another. But what restricts our present practice need not forever restrict our research. Probably more reliable, more meaningful, or more dynamically oriented systems of classifying patients can and should be developed. It is just as likely, however, that some critical thinking about the goals of diagnosis would lead to important developments in the prediction of individual behavior, in the appraisal of patients' treatability, and in the understanding of normal and deviant personality organization. It is even possible that a sober evaluation of the assumptions underlying diagnosis would lead to the conclusion that, in a good many situations, diagnosis as we traditionally conceive of it is unnecessary.

(*b*) We need diagnostic instruments constructed and employed in terms of explicit goals and philosophies. This applies equally to the old, familiar instruments, to the new, relatively untried ones, and to those which will be published next year. There is much information still to be collected about

the basic properties of our available instruments, particularly if we utilize them for ends other than diagnostic classification. It seems reasonably certain that one instrument which will turn out to be of greatest importance is one which has been most severely neglected, the clinician himself. Obviously, he cannot be excluded from any diagnostic situation, not even from the so-called "blind" analyses. The clinician is never blind. He is only astigmatic, to degrees and in directions which can be determined. Here again, contemporary research papers in psychodiagnosis recognize this problem. Statements regarding the necessity of "clinical experience" and "qualitative interpretation" in diagnosis appear in our publications with almost ritualistic regularity. Nobody doubts that the clinician as a person—trained or untrained, skillful or inept, obtuse or insightful—may be an important diagnostic instrument. He deserves closer and more critical scrutiny than we have yet given him.

(*c*) We need more comprehensive, planned research programs in diagnosis, each carefully oriented toward an important target. Partly because our diagnostic goals are confused and contradictory, our journals are surfeited with isolated studies, each unrelated to the others. The situation in the case of diagnostic studies using subtest analysis is a good illustration. Hundreds of such studies have now found their way into print, each differing enough from the others in patient sample, diagnostic preference, psychometric technique, or statistical analysis to prevent direct, meaningful comparisons among the obtained results. This is not to say that novel approaches to research problems should be discouraged. It is to say that, when there is a premium on novelty, research may become uneconomical and the results inconclusive. Much significant clinical research must necessarily be program-oriented rather than problem-oriented. We could obtain a great deal of badly needed information about diagnosis if we had more research programs, in which a relatively few subjects could be studied intensively, under controlled conditions, by a team of professional workers, with diverse diagnostic instruments, over a long period of time. If psychodiagnosis is as important as the number of publications about it would suggest, then such programs are not only possible but essential.

LITERATURE CITED

1. Abt, L. E., in *Projective Psychology*, 33–66 (Abt, L. E., and Bellak, L., Eds., Alfred A. Knopf, Inc., New York, 485 pp., 1950)
2. Abt, L. E., and Bellak, L., Eds., *Projective Psychology* (Alfred A. Knopf, Inc., New York, 485 pp., 1950)
3. Albee, G. W., and Goldman, R., *J. Projective Techniques*, **14**, 303–8 (1950)
4. Albee, G. W., and Hamlin, R. M., *J. Clin. Psychol.*, **6**, 363–64 (1950)
5. Andersen, A. L., *J. Clin. Psychol.*, **7**, 149–53 (1951)
6. Andersen, A. L., and Hanvik, L. J., *J. Clin. Psychol.*, **6**, 177–80 (1950)
7. Andrew, G., Walton, R. E., Hartwell, S. W., and Hutt, M. L., *J. Consulting Psychol.*, **15**, 51–54 (1951)
8. Beck, S. J., Rabin, A. I., Thiesen, W. G., Molish, H. B., and Thetford, W. N., *J. Psychol.*, **30**, 241–98 (1950)
9. Bellak, L., in *Progress in Neurology and Psychiatry*, 528–43 (Spiegel, E. A., Ed., Grune & Stratton, Inc., New York, 562 pp., 1950)
10. Bellak, L., in *Projective Psychology*, 7–32 (Abt, L. E., and Bellak, L., Eds., Alfred A. Knopf, Inc., New York, 485 pp., 1950)
11. Bellak, L., in *Projective Psychology*, 185–229 (Abt, L. E., and Bellak, L., Eds., Alfred A. Knopf, Inc., New York, 485 pp., 1950)
12. Bellak, L., *J. Projective Techniques*, **14**, 309–14 (1950)
13. Bellak, L., and Bellak, S. S., *J. Projective Techniques*, **14**, 173–80 (1950)
14. Benarick, S. J., Guthrie, G. M., and Snyder, W. U., *J. Consulting Psychol.*, **15**, 142–44 (1951)
15. Bills, R. E., *J. Clin. Psychol.*, **6**, 291–92 (1950)
16. Bills, R. E., Leiman, C. J., and Thomas, R. W., *J. Clin. Psychol.*, **6**, 293–94 (1950)
17. Blake, R. R., and Ramsey, G. V., Eds., *Perception: An Approach to Personality* (The Ronald Press Co., New York, 442 pp., 1951)
18. Blake, R. R., and Wilson, G. P., Jr., *J. Abnormal Social Psychol.*, **45**, 459–72 (1950)
19. Blum, G. S., *Genetic Psychol. Monographs*, **39**, 3–99 (1949)
20. Blum, G. S., *J. Abnormal Social Psychol.*, **45**, 536–37 (1950)
21. Botwinick, J., and Birren, J. E., *J. Consulting Psychol.*, **15**, 145–50 (1951)
22. Broida, D. C., Izard, C. E., and Cruickshank, W. M., *J. Clin. Psychol.*, **6**, 243–48 (1950)
23. Brower, D., and Weider, A., in *Projective Psychology*, 437–61 (Abt, L. E., and Bellak, L., Eds., Alfred A. Knopf, Inc., New York, 485 pp., 1950)
24. Brown, M. A., *J. Clin. Psychol.*, **6**, 266–69 (1950)
25. Bruner, J. S., in *Perception: An Approach to Personality*, 121–47 (Blake, R. R., and Ramsey, G. V., Eds., The Ronald Press Co., New York, 442 pp., 1951)
26. Buck, J. N., *J. Projective Techniques*, **14**, 405–34 (1950)
27. Bühler, C., *J. Projective Techniques*, **14**, 315–19 (1950)
28. Calvin, J. S., and Ward, L. C., *J. Clin. Psychol.*, **6**, 377–80 (1950)
29. Cameron, N., in *Perception: An Approach to Personality*, 283–306 (Blake, R. R., and Ramsey, G. V., Eds., The Ronald Press Co., New York, 442 pp., 1951)
30. Canter, A. H., *J. Gen. Psychol.*, **44**, 3–26 (1951)
31. Canter, A. H., *J. Gen. Psychol.*, **44**, 27–50 (1951)
32. Carp, A. L., and Shavzin, A. R., *J. Consulting Psychol.*, **14**, 230–33 (1950)
33. Carp, F. M., *J. Consulting Psychol.*, **14**, 268–75 (1950)

34. Cattell, R. B., and Luborsky, L. B., *J. Gen. Psychol.*, **42**, 3–24 (1950)
35. Challman, R. C., *Ann. Rev. Psychol.*, **2**, 239–58 (1951)
36. Clark, J. H., and Moore, J. H., *J. Consulting Psychol.*, **14**, 493–95 (1950)
37. Cohen, J., *J. Consulting Psychol.*, **14**, 371–75 (1950)
38. Cohen, J., *J. Consulting Psychol.*, **15**, 130–33 (1951)
39. Cole, D., and Roberts, E., *J. Clin. Psychol.*, **6**, 381–86 (1950)
40. Corsini, R., *J. Clin. Psychol.*, **7**, 127–30 (1951)
41. Cotzin, M., and Gallagher, J. J., *J. Consulting Psychol.*, **14**, 358–64 (1950)
42. David, H. P., *J. Consulting Psychol.*, **14**, 502 (1950)
43. David, H. P., *J. Abnormal Social Psychol.*, **45**, 735–37 (1950)
44. Dennis, W., in *Perception: An Approach to Personality*, 148–69 (Blake, R. R., and Ramsey, G. V., Eds., The Ronald Press Co., New York, 442 pp., 1951)
45. Deri, S. K., in *Projective Psychology*, 298–321 (Abt, L. E., and Bellak, L., Eds., Alfred A. Knopf, Inc., New York, 485 pp., 1950)
46. Derner, G. F., Aborn, M., and Canter, A. H., *J. Consulting Psychol.*, **14**, 172–79 (1950)
47. Dubrovner, R. J., VonLackum, W. J., and Jost, H., *J. Clin. Psychol.*, **6**, 331–36 (1950)
48. DuMas, F. M., *J. Gen. Psychol.*, **43**, 131–36 (1950)
49. DuMas, F. M., *J. Gen. Psychol.*, **43**, 137–42 (1950)
50. Ellis, R. W., and Brown, G. G., *J. Clin. Psychol.*, **6**, 298–99 (1950)
51. Eron, L. D., *Psychol. Monographs*, **64**(9), 1–48 (1950)
52. Eron, L. D., and Ritter, A. M., *J. Consulting Psychol.*, **15**, 55–61 (1951)
53. Eron, L. D., Terry, D., and Callahan, R., *J. Consulting Psychol.*, **14**, 473–78 (1950)
54. Estes, S. G., *Am. J. Psychiat.*, **107**, 514–15 (1951)
55. Fein, L. G., *J. Clin. Psychol.*, **6**, 248–52 (1950)
56. Fisher, S., *J. Projective Techniques*, **15**, 98–108 (1951)
57. Fisher, S., and Fisher, R., *J. Abnormal Social Psychol.*, **45**, 727–32 (1950)
58. Ford, M., *Univ. Minnesota Inst. Child Welfare Monogr.*, No. 23 (1946)
59. Fosberg, I. A., *J. Consulting Psychol.*, **15**, 39–44 (1951)
60. Fox, C., and Birren, J. E., *J. Consulting Psychol.*, **14**, 305–10 (1950)
61. Freeman, M. J., *J. Personality*, **19**, 229–43 (1950)
62. French, E. G., and Hunt, W. A., *J. Clin. Psychol.*, **7**, 95–97 (1951)
63. French, R. L., *J. Consulting Psychol.*, **14**, 111–15 (1950)
64. Frenkel-Brunswik, E., in *Perception: An Approach to Personality*, 356–420 (Blake, R. R., and Ramsey, G. V., Eds., The Ronald Press Co., New York, 442 pp., 1951)
65. Gerboth, R., *J. Consulting Psychol.*, **14**, 365–70 (1950)
66. Gibby, R. G., *J. Projective Techniques*, **15**, 3–26 (1951)
67. Gilhooly, F. M., *J. Consulting Psychol.*, **14**, 82–87 (1950)
68. Goldenberg, H. C., *J. Projective Techniques*, **15**, 79–86 (1951)
69. Gough, H. G., *J. Consulting Psychol.*, **14**, 408–13 (1950)
70. Grayson, H. M., and Tolman, R. S., *J. Abnormal Social Psychol.*, **45**, 216–31 (1950)
71. Guertin, W. H., *J. Clin. Psychol.*, **6**, 262–65 (1950)
72. Guertin, W. H., *J. Consulting Psychol.*, **14**, 404–7 (1950)
73. Guertin, W. H., *J. Clin. Psychol.*, **7**, 163–66 (1951)
74. Gurvitz, M. S., *J. Clin. Psychol.*, **6**, 397–400 (1950)

XUM

75. Gurvitz, M. S., *J. Clin. Psychol.*, **7**, 131–34 (1951)
76. Hamlin, R. M., Albee, G. W., and Leland, E. M., *J. Consulting Psychol.*, **14**, 276–82 (1950)
77. Hanvik, L. J., and Andersen, A. L., *J. Consulting Psychol.*, **14**, 197–98 (1950)
78. Harper, A. E., Jr., *J. Consulting Psychol.*, **14**, 290–96 (1950)
79. Harper, A. E., Jr., *J. Consulting Psychol.*, **14**, 351–57 (1950)
80. Harris, R. E., in *Diagnostic Psychological Testing*, 13–29 (Harrower, M. R., Ed., Charles C Thomas, Publisher, Springfield, Ill., 120 pp., 1950)
81. Harrower, M. R., Ed., *Diagnostic Psychological Testing* (Charles C Thomas, Publisher, Springfield, Ill., 120 pp., 1950)
82. Harrower, M. R., in *Projective Psychology*, 146–84 (Abt, L. E., and Bellak, L., Eds., Alfred A. Knopf, Inc., New York, 485 pp., 1950)
83. Harrower, M. R., *J. Clin. Psychol.*, **6**, 213–33 (1950)
84. Hartwell, S. W., Hutt, M. L., Andrew, G., and Walton, R. E., *Am. J. Orthopsychiat.*, **21**, 124–37 (1951)
85. Hathaway, S. R., *J. Consulting Psychol.*, **11**, 334–37 (1947)
86. Hathaway, S. R., and Meehl, P. E., Eds., *An Atlas for the Clinical Use of the MMPI* (Univ. Minnesota Press, Minneapolis, 799 pp., 1951)
87. Hewson, L. R., *J. Nervous Mental Diseases*, **109**, 158–83, 246–66 (1949)
88. Hilgard, E. R., in *Perception: An Approach to Personality*, 95–120 (Blake, R. R., and Ramsey, G. V., Eds., The Ronald Press Co., New York, 442 pp., 1951)
89. Hire, A. W., *J. Consulting Psychol.*, **14**, 496–99 (1950)
90. Holt, R. R., *J. Projective Techniques*, **14**, 435–44 (1950)
91. Holtzman, W. H., *J. Clin. Psychol.*, **6**, 343–47 (1950)
92. Holtzman, W. H., *J. Clin. Psychol.*, **6**, 348–51 (1950)
93. Holzberg, J. D., and Deane, M. A., *J. Consulting Psychol.*, **14**, 180–88 (1950)
94. Holzberg, J. D., and Wexler, M., *J. Projective Techniques*, **14**, 343–61 (1950)
95. Holzberg, J. D., and Wexler, M., *J. Consulting Psychol.*, **14**, 395–99 (1950)
96. Howard, A. R., *J. Consulting Psychol.*, **14**, 376–80 (1950)
97. Hughes, R. M., *J. Gen. Psychol.*, **43**, 85–104 (1950)
98. Hunt, H. F., *Ann. Rev. Psychol.*, **1**, 207–20 (1950)
99. Hunt, W. A., Wittson, C. L., and Burton, H. W., *J. Consulting Psychol.*, **14**, 35–39 (1950)
100. Hunt, W. A., Wittson, C. L., and Burton, H. W., *J. Consulting Psychol.*, **14**, 485–88 (1950)
101. Hunt, W. A., French, E. G., Klebanoff, S. G., Mensh, I. N., and Williams, M., *J. Consulting Psychol.*, **12**, 171–73 (1948)
102. Hunt, W. A., French, E. G., Klebanoff, S. G., Mensh, I. N., and Williams, M., *J. Consulting Psychol.*, **12**, 355–59 (1948)
103. Hutt, M. L., Gibby, R., Milton, E. O., and Pottharst, K., *J. Projective Techniques*, **14**, 181–87 (1950)
104. Kadis, A. L., in *Projective Psychology*, 403–34 (Abt, L. E., and Bellak, L., Eds., Alfred A. Knopf, Inc., New York, 485 pp., 1950)
105. Kates, S. L., *J. Consulting Psychol.*, **14**, 226–29 (1950)
106. Kelley, H. H., *J. Personality*, **18**, 431–39 (1950)
107. Kelly, E. L., and Fiske, D. W., *The Selection of Clinical Psychologists: Progress Report and Preliminary Findings of the Research Project on the Selection of Clinical Psychologists* (Univ. Michigan, Ann Arbor, 56 pp., 1948)
108. Kimball, A. J., *J. Projective Techniques*, **14**, 134–52 (1950)

109. Kimball, A. J., *J. Projective Techniques*, **14**, 219–44 (1950)
110. Kitay, J. I., *J. Clin. Psychol.*, **6**, 170–74 (1950)
111. Kitzinger, H., and Blumberg, E., *Psychol. Monographs*, **65**(2), 1–20 (1951)
112. Klein, G. S., in *Perception: An Approach to Personality*, 328–55 (Blake, R. R., and Ramsey, G. V., Eds., The Ronald Press Co., New York, 442 pp., 1951)
113. Klein, G. S., and Schlesinger, H. J., *Am. Psychologist*, **5**, 231 (1950)
114. Klopfer, W. G., and Borstelmann, L. J., *J. Personality*, **19**, 172–88 (1950)
115. Knott, J. R., Cannicoot, R. G., Umberger, J. P., and Bilodeau, I. M., *J. Clin. Psychol.*, **7**, 123–26 (1951)
116. Kogan, W. S., *J. Gen. Psychol.*, **43**, 17–46 (1950)
117. Korchin, S. J., Mitchell, H. E., and Meltzoff, J., *J. Projective Techniques*, **14**, 445–52 (1950)
118. Korner, A. F., *Some Aspects of Hostility in Young Children* (Grune & Stratton, Inc., New York, 194 pp., 1949)
119. Korner, A. F., *J. Abnormal Social Psychol.*, **45**, 619–27 (1950)
120. Levy, S., in *Projective Psychology*, 257–97 (Abt, L. E., and Bellak, L., Eds., Alfred A. Knopf, Inc., New York, 485 pp., 1950)
121. Lindner, R. M., in *Projective Psychology*, 75–90 (Abt, L. E., and Bellak, L., Eds., Alfred A. Knopf, Inc., New York, 485 pp., 1950)
122. Lord, E., *Psychol. Monographs*, **65**(10), 1–34 (1950)
123. Lowenfeld, M., *J. Psychol.*, **30**, 325–31 (1950)
124. Luft, J., *J. Abnormal Social Psychol.*, **45**, 756–59 (1950)
125. Matarazzo, J. D., *J. Clin. Psychol.*, **6**, 337–42 (1950)
126. McLeod, H., *J. Projective Techniques*, **14**, 453–63 (1950)
127. McNemar, Q., *J. Consulting Psychol.*, **14**, 79–81 (1950)
128. McReynolds, P., *J. Abnormal Social Psychol.*, **46**, 131–41 (1951)
129. Meehl, P. E., *J. Consulting Psychol.*, **14**, 165–71 (1950)
130. Meer, B., and Singer, J. L., *J. Consulting Psychol.*, **14**, 482–84 (1950)
131. Meltzer, H., *J. Personality*, **19**, 48–63 (1950)
132. Meltzoff, J., *J. Abnormal Social Psychol.*, **46**, 177–89 (1951)
133. Mercer, M., and Wright, S. C., *J. Projective Techniques*, **14**, 287–96 (1950)
134. Miller, J. G., in *Diagnostic Psychological Testing*, 3–12 (Harrower, M. R., Ed., Charles C Thomas, Publisher, Springfield, Ill., 120 pp., 1950)
135. Miller, J. G., in *Perception: An Approach to Personality*, 258–82 (Blake, R. R., and Ramsey, G. V., Eds., The Ronald Press Co., New York, 442 pp., 1951)
136. Morris, W. W., and Nicholas, A. L., *J. Clin. Psychol.*, **6**, 309–14 (1950)
137. Muench, G. A., *Applied Psychol. Monogr.*, No. 13 (1947)
138. Muench, G. A., in *Diagnostic Psychological Testing*, 53–72 (Harrower, M. R., Ed., Charles C Thomas, Publisher, Springfield, Ill., 120 pp., 1950)
139. Munroe, R. L., in *Projective Psychology*, 91–145 (Abt, L. E., and Bellak, L., Eds., Alfred A. Knopf, Inc., New York, 485 pp., 1950)
140. Neff, W. S., and Lidz, T., *J. Projective Techniques*, **15**, 45–57 (1951)
141. Newton, R. L., *J. Clin. Psychol.*, **6**, 409–11 (1950)
142. Ochs, E., *J. Clin. Psychol.*, **6**, 282–84 (1950)
143. Osborne, R. T., and Sanders, W. B., *J. Clin. Psychol.*, **6**, 258–61 (1950)
144. Pascal, G. R., *Am. J. Orthopsychiat.*, **20**, 418–23 (1950)
145. Pascal, G. R., and Suttell, B. J., *The Bender-Gestalt Test* (Grune & Stratton, Inc., New York, 288 pp., 1951)
146. Perlman, J. A., *J. Consulting Psychol.*, **15**, 122–26 (1951)

147. Precker, J. A., *J. Projective Techniques*, **14**, 262–86 (1950)
148. Proshansky, H. M., in *Projective Psychology*, 462–84 (Abt, L. E., and Bellak, L., Eds., Alfred A. Knopf, Inc., New York, 485 pp., 1950)
149. Rabin, A. I., *J. Abnormal Social Psychol.*, **45**, 392–95 (1950)
150. Rabin, A. I., *J. Consulting Psychol.*, **14**, 400–3 (1950)
151. Rabin, A. I., and Beck, S. J., *Am. J. Orthopsychiat.*, **20**, 595–99 (1950)
152. Rabin, A. I., and Guertin, W. H., *Psychol. Bull.*, **48**, 211–48 (1951)
153. Ranzoni, J. H., Grant, M. Q., and Ives, V., *J. Projective Techniques*, **14**, 107–33 (1950)
154. Rappaport, S. R., and Webb, W. B., *J. Consulting Psychol.*, **14**, 95–98 (1950)
155. Reiman, M. G., *Am. J. Orthopsychiat.*, **20**, 600–15 (1950)
156. Riess, B. F., Schwartz, E. K., and Cottingham, A., *J. Abnormal Social Psychol.*, **45**, 700–9 (1950)
157. Roe, A., *J. Consulting Psychol.*, **13**, 225–46 (1949)
158. Roe, A., *J. Projective Techniques*, **14**, 385–98 (1950)
159. Roe, A., *Genet. Psychol. Monographs*, **43**, 121–235 (1951)
160. Rogers, L. S., *J. Consulting Psychol.*, **15**, 151–53 (1951)
161. Rosenberg, S., *J. Projective Techniques*, **14**, 188–93 (1950)
162. Rosenzweig, S., *J. Psychol.*, **30**, 139–43 (1950)
163. Rosenzweig, S., *J. Abnormal Social Psychol.*, **45**, 507–9 (1950)
164. Rotter, J. B., *J. Abnormal Social Psychol.*, **46**, 277–78 (1951)
165. Sacks, J. M., and Levy, S., in *Projective Psychology*, 357–402 (Abt, L. E., and Bellak, L., Eds., Alfred A. Knopf, Inc., New York, 485 pp., 1950)
166. Salfield, D. J., *J. Gen. Psychol.*, **43**, 305–12 (1950)
167. Sanderson, H., *J. Consulting Psychol.*, **15**, 127–29 (1951)
168. Sarason, S. B., *J. Clin. Psychol.*, **6**, 387–92 (1950)
169. Schwartz, A. A., *J. Projective Techniques*, **14**, 153–72 (1950)
170. Seward, J. P., *J. Abnormal Social Psychol.*, **45**, 529–35 (1950)
171. Siipola, E., Kuhns, F., and Taylor, V., *J. Personality*, **19**, 153–71 (1950)
172. Spache, G., *J. Clin. Psychol.*, **6**, 406–8 (1950)
173. Stacey, C. L., and Portnoy, B., *J. Clin. Psychol.*, **7**, 144–48 (1951)
174. Thetford, W. M., Molish, H. B., and Beck, S. J., *J. Projective Techniques*, **15**, 58–78 (1951)
175. Thompson, C. E., *Rorschach Research Exch.*, **13**, 469–78 (1949)
176. Thompson, G. M., *J. Consulting Psychol.*, **14**, 287–89 (1950)
177. Tucker, J. E., *J. Consulting Psychol.*, **14**, 283–86 (1950)
178. Tupes, E. C., *Psychol. Monographs*, **64**(11), 1–24 (1950)
179. Waldfogel, S., and Guy, W., *J. Clin. Psychol.*, **7**, 135–39 (1951)
180. Warner, S. J., *J. Consulting Psychol.*, **14**, 297–304 (1950)
181. Wauck, L. A., *J. Clin. Psychol.*, **6**, 279–81 (1950)
182. Weider, A., and Noller, P. A., *J. Clin. Psychol.*, **6**, 319–25 (1950)
183. Wertham, F., in *Projective Psychology*, 230–56 (Abt, L. E., and Bellak, L., Eds., Alfred A. Knopf, Inc., New York, 485 pp., 1950)
184. Wheeler, J. I., Jr., and Wilkins, W. L., *J. Consulting Psychol.*, **15**, 163–66 (1951)
185. Wheeler, W. M., Little, K. B., and Lehner, G. F. J., *J. Consulting Psychol.*, **15**, 134–41 (1951)
186. Wiltshire, H. M., *J. Clin. Psychol.*, **7**, 167–70 (1951)
187. Winne, J. F., *J. Clin. Psychol.*, **7**, 117–22 (1951)
188. Wittenborn, J. R., *J. Consulting Psychol.*, **14**, 216–25 (1950)

189. Wittenborn, J. R., *J. Consulting Psychol.*, **14**, 261–67 (1950)
190. Wittenborn, J. R., *J. Consulting Psychol.*, **14**, 469–72 (1950)
191. Wittenborn, J. R., *J. Consulting Psychol.*, **14**, 500–1 (1950)
192. Wittenborn, J. R., and Eron, L. D., *J. Consulting Psychol.*, **15**, 45–50 (1951)
193. Wittson, C. L., and Hunt, W. A., *Am. J. Psychiat.*, **107**, 582–85 (1951)
194. Woltmann, A. G., in *Projective Psychology*, 322–56 (Abt, L. E., and Bellak, L., Eds., Alfred A. Knopf, Inc., New York, 485 pp., 1950)
195. Zubin, J., in *Diagnostic Psychological Testing*, 99–120 (Harrower, M. R., Ed., Charles C Thomas, Publisher, Springfield, Ill., 120 pp., 1950)
196. Zubin, J., *et al.*, *J. Clin. Psychol.*, **6**, 1–76 (1950)

CLINICAL METHODS: PSYCHOTHERAPY[1]

By Victor C. Raimy

Department of Psychology, University of Colorado, Boulder, Colorado

Introduction

As one surveys the past year's output of material on psychotherapy, one gains four distinct impressions. First of all, there seems to be an enormous amount of interest and vitality among those persons who are working in the field of psychotherapy. Psychologists and psychiatrists are quite clearly bursting with ideas, anxious to discuss their impressions, methods, and plans. Secondly, one must mention the great lack of any systematic framework around which most of the writers can organize their ideas. Aside from that of the psychoanalysts and the nondirectivists, most of the literature deals with isolated fragments of ideas and fragments of either clinical or objective research. Appeals to authority, appeals to tradition, speculation about vaguely phrased new departures, polemics without foundation, and careful criticisms without supporting data characterize most of the field. Thirdly, the emphasis placed upon one form or another of group psychotherapy leaves little doubt that this comparatively new field has gained a place of its own, despite the lack of data to show that it is any more effective than the individual approaches or even as effective. One could wish that climbing on the bandwagon of group psychotherapy might be done with a little less disregard of the problems involved, if only to save the interested reader some valuable time. Fourthly, one searches almost in vain for some factual data concerning the outcome of most of the methods used in attempting to cure (or reorganize or readjust) the patients or clients who have been treated. While recognizing that at present adequate methods for assessing change in personality are almost impossible to devise, one nonetheless would like some indication—preferably accompanied by objectively-obtained, though perhaps debatable, statistics—as to what happens to those under treatment.

Thus, in preliminary summary, the field of psychotherapy appears to have an enormous but somewhat leaderless vitality. Research plays little part; theory is largely at the clinical level; and speculation as to underlying processes and relevant factors reigns supreme. Descriptions of methods of treatment are largely confined to anecdotal presentations or discussions of presumed principles conducted at a considerable distance from the operational sphere. Finally, thousands and thousands of persons are being treated in one fashion or another, but no one seems very certain what the outcome has been or will be.

At this point, and parenthetically, it might be worthwhile to mention that the appalling lack of follow-up in the treatment of mental disturbances certainly con-

[1] This review covers the period from approximately April, 1950 to April, 1951. The writer wishes to express his gratitude to N. Elaine Davis and Jerry M. Allen, Jr. for their assistance in its preparation.

tributes to the lack of knowledge, the insistence upon personal predilections, and the general impression that no one knows what is really going on. This sounds like a very critical statement and it is. However, anyone who, like the writer, has attempted to do follow-up on persons treated by psychotherapy can appreciate the enormous difficulties involved. Granting the difficulties, one can nonetheless hope that someone, somewhere, will develop methods permitting at least reasonable assessment of outcomes. The physical and medical sciences have been largely revolutionized by their great good fortune of being able to study the effects of their experimental efforts. Basic conceptions of mental deficiency and delinquency have been revolutionized during the past 30 years by only a very few follow-up studies. What would happen to many if not all of the current ideas in psychotherapy if the same could be done in this field, is perhaps not pleasant to contemplate.

Thus the tone of the present review, as far as the history of this science is concerned, is pessimistic. My attitude, however, is not pessimistic, as I am basically starting by making a sincere curtsy to the facts. Actually, the scientific method has always been a positive avowal of faith in the future, a willingness to permit any idea access to the theater of scientific action, and an insistence upon recognizing present reality. The situation outlined above represents present reality. However, if only because of the great distance we must go in creative conception and effective methodology, the prospect is not discouraging, but rather exciting. At the least, most psychotherapists seem to feel excited by the possibilities which they can but vaguely sense. Some psychotherapists are just excited, of course, but the vast majority are dealing sincerely with what appears to be the beginning stages of a most difficult science. The work of the technician who laboriously checks the hypotheses of the most creative thinkers is but rarely found in the writings on psychotherapy. Even the few technicians, however, seemingly feel that they are a part of a dramatic new endeavor.

There are two areas of psychotherapy in which some order reigns, in which the workers are at least comfortable in their faith and to which not all of the above criticisms apply. In psychoanalysis, which is now largely dominating the psychiatric scene, at least insofar as terminology is concerned, the thinking is relatively consistent, the efforts are less scattered, and the whole atmosphere is redolent of certainty—almost too much certainty, one feels, until the works of the Neo-Freudians are considered. Also among the nondirectivists there is a sense of direction: the early effusions have died down, there is a recognition of other psychotherapies, aggressions from and toward the group have practically disappeared, and the keynote has become largely one of research. True, their research is largely a search for methods, and their results are circumscribed and fragmentary; yet, as a group, the nondirectivists are perhaps more serious about seeking objective verification than are most others in the psychotherapeutic fraternity.

In final summary, one further point is of interest. The journal articles, which are apparently less numerous than in the immediately preceding years, are, for the most part, singularly unrewarding reading except for the psychotherapist or researcher who cannot afford to miss any opportunity to find a potentially good idea. The published books, on the other hand, reveal some of the real progress which is being made in this field. In fact, it is quite likely that the past year has seen the publication of at least three really significant contributions to psychotherapy. No one of the three stands out above the other two, as each of them deals with a different portion of the field. Each of them will be reviewed rather extensively below, since they are in themselves significant summaries of their own territory.

The first of these, Dollard & Miller's *Personality and Psychotherapy* (13),

is a culmination of the yearnings of many clinical psychologists and some academic psychologists who have long wanted to bring about a wedding between learning theory and psychotherapy. The second book, Rogers' *Client-Centered Therapy* (59), is not merely a revision of his earlier *Counseling and Psychotherapy* (58), although the two are more than historically related. In addition to possessing a new name and new visions, the later book also summarizes and integrates the research of the nondirectivists, almost all of which appeared after the publication of the earlier *Counseling and Psychotherapy*. Furthermore, a theoretical foundation has been added which unites the nondirectivists with the many contemporary psychologists who are reviving interest in the empirical self of William James.

The third book, *Intensive Psychotherapy* by Fromm-Reichmann (23), has less inherent significance than the other two, perhaps because it is only a part of a much larger revolt against classical Freudianism, and also because it is written less systematically. Her presentation of Sullivan's theory of interpersonal-relationships (68) is, however, much more readable than that of Sullivan. Furthermore, she boldly desexualizes the Oedipus complex, she de-emphasizes the basic rule, and she advocates the chair in preference to the couch. Although the Freudian firmament is still in little danger of disintegrating, Fromm-Reichmann's book clearly indicates that workmen are busily shifting the segments about, removing some apparently loose members, and trying to add a new coat of paint. They are, of course, being seriously interfered with by other workmen who much prefer the old and conceivably more comfortable canopy.

Other books of real stature have appeared and will be mentioned throughout the course of this review.

Objective Research

Although it may seem a little unusual to devote a separate section to research, the condition of objective research in psychotherapy seems to justify such a division, largely because there is so little. Those studies which have appeared during the past year therefore probably deserve highlighting in this fashion.

One should mention in advance that all objective research studies are handicapped in design by the tremendous expense of the processes involved in obtaining and studying the raw data of psychotherapy. Nonetheless, one-way vision screens, phonographic recordings, sound-motion pictures, and perhaps even television are steadily producing technological conditions which make such research more and more feasible. If only the investigators knew what aspects of the process they should focus their attention upon, the results would probably be much more fruitful. Furthermore, even when interesting methods are devised and full advantage is taken of technical equipment, the problem of aligning patients and therapists with a research design introduces practical problems which so far have discouraged all but the most ardently research-minded students of psychotherapy.

One of the barriers to research in this area which has received but little

explicit attention in the past might well be brought into the open at this point. The barrier referred to is an attitude on the part of psychotherapists which has made the past 25 years a period which might well be labeled the "egg-shell era." Almost without exception (although there were some men, such as Alfred Adler and Ira Wile, who were not unwilling to conduct psychotherapy in public), psychotherapists have adopted the attitude that patients and clients were frail, puny beings who would flee the field if anyone except their very own private psychotherapist touched them. In view of the lack of evidence to the contrary, such an attitude was at one time entirely justified, since, in simple ethics, the welfare of the patient does come first. The fact that the attitude also had other supports, particularly the fact that it provided defensive measures for the therapists' unsteady ego-structure, made no difference. However, now there is increasing evidence that at least some patients will not be harmed or disturbed by the introduction of research devices such as sound recorders, observers, other patients, other therapists, etc.

Group therapists, for example, seem to have shown that recording devices can be successfully introduced into the therapy process itself without undue disturbance to the members of the group. In the past year, both Luchins (39) and McCann & Almada (42) report that sound recorders were used as part of the process of group therapy, recordings being played back to the group immediately after the sessions. In fact, the two latter writers limited the length of their sessions to 1/2 hr., since their recorder ran only that long! Grotjahn (28) advocates that the supervisor of the group therapist should actually participate in the group at times in order to facilitate supervision, and Whitaker *et al.* (76) report successful results following the inviting of a consultant therapist into individual therapy sessions when an impasse occurs. Even more unconventional is the practice of having two or even more therapists present during the whole course of therapy for a single patient. This so-called "multiple therapy," originally described by Whitaker *et al.* (75), is more recently discussed by Haigh & Kell (29). Rogers (59) says that "with some clients it may even facilitate therapy."

Thus the privacy of the therapeutic session has been invaded by recording devices, by supervising therapists, by an increased number of patients (with a single therapist), and by one or even two added therapists for a single patient; and apparently the patients or clients have not been harmed or disturbed thereby. It may be that when patients have been disturbed by such innovations, the therapists continued in ignorance or failed to report the results. However, it seems more likely, particularly in view of the frankness with which persons in group therapy discuss their intimate attitudes [see Baruch & Miller (4) and Stone & Levine (66)], that psychotherapists have previously been interpreting their own defensive fears in terms of the supposed welfare of the patient.

This is certainly not to urge that the wraps be taken completely off the therapeutic sessions nor that the privacy of either the therapist or the patient be invaded when it would be to the detriment of the patient. An extremely

cautious approach is still the most feasible as well as the most ethical, but some attention should certainly be given to formal and informal research on this problem. In psychoanalysis, for example, the emotional character of the transference might well prohibit the introduction even of recording devices. Analysts could be expected to ascertain, however, that it is the patient's transference and not the analyst's counter-transferences which prohibits the use of research devices. Ideally, we could hope that psychotherapists could be as free as surgeons in inviting qualified onlookers to observe them at work. The goal may be impossible, but any steps in that direction would certainly remove some of the mystery from psychotherapy.

The past year has resulted in two particularly outstanding pieces of objective research, one on schizophrenics and one on psychotherapists themselves. Gottlieb & Huston (26), psychiatrists at the Iowa Psychopathic Hospital, compared the effectiveness of three methods for treating early, undeteriorated schizophrenics. The three methods were brief psychotherapy (averaging seven weeks), insulin coma, and electric shock. Patient groups were roughly equated in terms of age, sex, length of hospitalization, schizophrenic sub-type, and racial composition. Unfortunately, the authors failed to state why 128 patients were assigned to psychotherapy, 65 patients to insulin, and 143 to electroconvulsive shock therapy (ECT). Their results should be of extreme interest to those working with functional psychotics, since they failed to find any statistically significant differences among the three groups during any of the successive follow-up periods (the shortest period of follow-up was one year and some patients were followed for four years). They state:

> These comparisons do not reveal any superiority of insulin or electric shock in the treatment of schizophrenia. This comment applies to either temporary or more permanent amelioration of the psychosis. Neither of these two methods is superior to the results with brief psychotherapy two years after hospitalization.

They further state: "Acuteness of onset is of more importance for recovery than the methods of therapy." Furthermore, when sub-type (e.g., simple, catatonic, etc.) was considered, "this variable is of greater importance for recovery than any of the methods of treatment employed." Reliable differences were found in recovery rate of catatonics (47 per cent) and paranoids (40 per cent) when compared with simple (23 per cent) and hebephrenics (28 per cent), regardless of treatment method. Although for humanitarian reasons it was impossible to add a fourth, non-treated control group, such an addition to the design would have been extremely helpful in determining whether any method of treatment is useful in speeding recovery or accomplishing remission. This question is particularly important when one considers the fact that recovery was observed during the follow-up period in only about 37 per cent of all the patients. The study raises a number of intriguing questions, not the least important of which is why have recovery rates from the shock therapies been falling so drastically in recent years as compared with 1938 or 1940? Is the human schizophrenic adapting to such procedures, much as the house fly is apparently resisting DDT? Or would small sums of

money spent on follow-up and other research procedures provide more adequate knowledge of the efficacy of current treatment methods in mental disorders?

Fiedler published an integrated series of three articles (19, 20, 21) reporting a provocative study of the attitudes and actions of psychoanalytic, Adlerian, and nondirective therapists in relation to their patients. Utilizing Stephenson's Q-technique (inverted factor analysis) which has been only poorly reported in the literature but which seems to be a major advance in research methodology, Fiedler (19) had very small groups of therapists of the three persuasions sort statements concerning good and bad therapeutic relationships. The correlations showed that "the better-trained therapists of different schools agreed more highly with each other than they agreed with less well-trained therapists within their own school." Furthermore "the findings suggest that therapists generally agree on the most effective type of therapeutic relationship." Nonetheless, naive subjects (nontherapists) who sorted the cards under the same instructions correlated positively and highly with the therapists' opinions but not as highly as did the therapists with one another. From these results, Fiedler concluded that a good therapeutic relationship is similar to any other good interpersonal relationship. Unfortunately, the "naive subjects" were apparently not overly naive so far as psychotherapy was concerned. It would be interesting, and perhaps not too difficult, to repeat part of his study, drawing on therapists who are not as closely related in their thinking as are Freudians, Adlerians, and nondirectivists.

Fiedler in his first article studied only the opinions of therapists in regard to the ideal relationship; his second article (20) reports a direct comparison of the attitudes displayed by a small number of therapists of each of the three schools. Four judges, three of whom were trained in some form of therapy, listened to one recorded interview from each of the therapists. Again the therapists were selected so as to have each school represented by "experts" and "non-experts." The judges rated the interviews in terms of the statements which had been sorted by the therapists in the preceding study. The findings indicate that insofar as "communication" (defined as "the therapist's ability to understand, to communicate with, and to maintain rapport with the patient") is concerned, the experts from all three schools are not only very similar but also more like each other than they are similar to nonexperts of their own schools. However, differences between schools did show up in two other aspects of relationship: the "emotional distance" between patient and therapist, and the "status role" which the therapist adopts.

This second article, then, seems to indicate what might be feared: Fiedler's first study, which showed such close agreement between experts of different schools, might well be an artifact of the therapists' public attitudes masking their private activities. Fiedler's contention that "communication" is the most important dimension of "good" therapy cannot be accepted without a prior evaluation of therapeutic effectiveness.

Without detracting from the ingenuity and careful planning shown in this series of articles, it should be mentioned that the radical differences

among therapists which Fiedler set out to investigate have been suspect for some time. The 1940 symposium of the American Orthopsychiatric Association (69) dealt with areas of agreement in psychotherapy. At that time, it appeared that most therapists could agree verbally with each other insofar as attitudes were concerned. Fiedler's work is thus something of an objective test of the univocality of the terms used and agreed to in that symposium.

In his third article (21), Fiedler presents a factor analysis of the Q-sorts of the four judges from the previous study of attitudes displayed by therapists in recorded interviews. Although Fiedler states that "no factors were found which clearly separate therapists of one school from those of another," he also states that

> while we have found several factors which clearly differentiate experts from non-experts, only one of the factors has differentiated three therapists of one school from those of another school.

The second quotation evidently refers to a factor

> which differentiates three of the four nondirective therapists from those of other schools. Nondirective therapists are here described (by inspection of the items with high factor loadings) as being less status conscious; some of the psychoanalytic therapists and the Adlerians, as can be expected, are seen as more authoritarian, tutorial, and with greater tendency to guide and direct the patient.

Although the quotation refers to only one of the 16 factors obtained (four for each judge with an unknown degree of overlap), the statement is, to use Fiedler's words, what might be expected. This finding inevitably raises the question: What are the major dimensions of a relationship? Are all of the 16 obtained factors equally important, or can we ignore some and not others?

The fairly simple hypothesis of Barry's study of maladjusted college students (3) holds that patients who spend the interview hour talking about things which threaten them, such as symptoms, inferiorities, and other felt deficiencies, are farther away from the end point of therapy than those who spend the hour discussing favorable aspects of themselves and their environments. This hypothesis is applied to electrically-recorded, evaluative interviews, initial and terminal, of college students who were considered "severely maladjusted, personally and socially, and their efficiency . . . impaired." Therapy had previously been conducted by Morton, who reported his results in another publication (46). The therapy is said to have been relatively directive.

Barry's results indicate that "expressions of perceived strength (and threat) were found to be associated very significantly with ratings of adjustment level." Adjustment level was gauged by having judges rate both the initial and terminal interviews, and also by a follow-up interview conducted in an open-ended survey fashion by Barry. He felt the criterion was relatively external as compared with that of earlier studies, which used the judgment of the therapist at the conclusion of therapy.

Barry's study seems to be chiefly significant for the finding that methods similar to those previously used to evaluate nondirective therapy are apparently applicable also to the more directive methods of treatment. He points out that his "strength-threat" categories are similar to Raimy's (54) "positive and negative self-evaluations," and also to Dollard & Mowrer's "relief and distress DRQ." Barry's study also adds another link to the by now rather extensive chain of evidence that changes in self-evaluation are at the very least concomitant with changes occurring in successful psychotherapy. For further studies on this point from the Chicago Counseling Center, see also the summary by Raskin (57). Unfortunately, most of the work to date, with the exception of Hunt's (33), has been done on persons who are primarily maladjusted college students. It would certainly be valuable to apply one of these similar methods to more serious problem cases in which successful therapy has occurred.

Although Collier (10) does not report any actual research data, his article is a preliminary report of what will probably be a rather extensive investigation. He suggests that the effective variables in therapist technique can be arranged on continua in order to eliminate apparent conflicts between various schools of thought. Once identification of the variables is achieved by placing specific techniques at their appropriate place on the continua, research applications of specific techniques to specific problems can be attempted. The continuum notion has also been suggested by Bordin (6) and Strang (67). Such an assumption, however, is not necessarily valid, although it may satisfy the psychologist's yearning to deal with bell-shaped curves. What may appear to the observer as a continuously-varying sequence of events may, to a client or patient, appear as a collection of quite different and unrelated experiences. Ultimate judgments concerning therapeutic technique must rest upon the effect which a particular technique has upon individuals undergoing treatment. The therapist's taking "a small amount of responsibility" may mean just that to the therapist. But does it mean the same thing for the patient who has a different set of attitudes and expectancies? The present objection, of course, is in itself an argument for the attempted identification of the assumed continua if only to determine whether the underlying assumption which Collier, Strang, and Bordin propose is valid.

Mensh & Watson (43) report a study of 18 psychiatrists' opinions on the goals of therapy and on those personality characteristics most likely to yield to treatment. There appears to be high agreement that change could be expected among persons who were emotionally inhibited or immature or unassertive. The influence of practice on hopes is indicated by the fact that the experienced psychiatrists agreed among themselves that factors of rigidity, inadequacy, and compulsiveness were unlikely to be changed by treatment, whereas the residents thought that those factors could be changed.

In another study (44), Mensh & Golden, after analyzing the closed records of 638 veterans who had been seen in a Veteran's Administration, Mental Hygiene Contract Clinic, were unable to find significant differences

between pension applicants and veterans who did not file for pensions. The variables studies related both to background and to clinic behavior. Also, when the patients were grouped into three categories—psychoneurotics, character disorders, and schizophrenics—the group with character disorders stood midway between the neurotics and schizophrenics on most of the variables.

Also dealing with a population of veterans from a Mental Hygiene Clinic, Dengrove & Kutash (12) present a thoughtful and objective study of why patients discontinue treatment. While no new explanations are to be found in their list of 16 categories, their objective presentation amply illustrates the fact that a patient in a clinic is interacting with therapists and with clinic procedures. Discontinuance of treatment apparently cannot be explained only on the basis of resistance and lack of motivation on the part of the patient.

Three studies attempting to use tests to measure outcomes of psychotherapy should be mentioned. Schofield (62), using the Minnesota Multiphasic Personality Inventories (MMPI) found in the records of the research files at the University of Minnesota, apparently had extreme difficulty in finding criteria and in describing treatment. His investigation shows a resolute determination to keep it methodologically clean, but his results are largely disappointing. Psychotic patients treated with electric shock (mostly depressives and involutionals) did show significant reductions on most MMPI scales. His hospitalized neurotics also showed significant decreases after one month of intramural treatment, but they lacked later follow-up. The outpatients, who averaged about five clinic visits between tests, failed to show any signicant changes.

Kaufman (35), studying the MMPI's of 51 college students treated at a university health service, compared them with a control group drawn from psychology classes. He found significant differences on some scales for the patient group before and after treatment, but a significant change only on the K-scale for the controls.

Lord (38) adds another bit of evidence to the current controversy over whether the Rorschach Test measures changes resulting from psychotherapy. With only two subjects (each of whom had six months of "brief psychotherapy"), and without controls, she found that there is a constancy of "basic personality configurations despite successful brief psychotherapy." However, she also claims that changes in Rorschach protocols in the direction of improved balance and community thinking were detected.

Research to date on the use of psychometric and projective tests as a means of assessing psychotherapy can be summed up by the statement that when adequate external criteria are available, psychologists will be able to determine whether their tests can detect changes in personality. This does not apply, of course, to the very gross changes in personality functioning.

It is questionable whether all the objective research to date has added much to our knowledge of psychotherapy. Certainly, the few good follow-

up studies have enlightened us enormously as to the efficacy of psychotherapeutic efforts. One might well ask whether objective research is worth-while at this time. Couldn't the time be better spent in more intensive as well as broader training in psychotherapy; in more and better clinical analysis?

Were it not for the peculiar nature of the field itself, objective research might well be postponed until the time when relevant and significant variables have been refined out of the present mélange of private insights, excited hypothesizing, ardent application, and intercult competition. However, as Muncie writes (49).

> One may learn from several years of experience, there is no sure way of dealing with any psychiatric problem. Failures with any and all methods leave one with the only possible choice, namely, to be plastic and free of biases that have grown up through tradition or teaching at the hands of even the most authoritative figures.

There is little doubt that psychotherapists have substituted and are substituting authority and doctrine for the much more trying task of intercourse with reality: a state of affairs that might best be ameliorated by continuation and extension of activity in the area of objective research.

Training

Although it now seems that almost everyone is agreed that clinical psychologists should be trained in psychotherapy, there still remains considerable doubt in some quarters whether they should perform psychotherapy. Aside from one symposium, the year has seen a very great diminution in the amount of emotion displayed in public discussion. The one symposium (25) mentioned is about as forthright a discussion as one could hope to find and, in reading it, one fears that at any moment both participants will accuse each other of being un-American. Another symposium (79), held during the 1949 meeting of the American Orthopsychiatric Association deals more temperately with the problem. Gardner, one of the participants, presents a trenchant discussion of some technical matters in training for therapy. Although sympathetic towards the idea that intensive training in therapy for clinical psychologists should come after the Ph.D., he feels that third-year trainees should nonetheless carry at least six cases in treatment under supervision. He also mentions that the greatest need at the present time is for supervisory (training) personnel who are themselves psychologists and suggests that clinical psychologists should devise some plan for training more supervisors.

The book, *Training in Clinical Psychology*, a report of an American Psychological Association conference in 1949 (55), devotes one chapter to "Training for Psychotherapy," in which it is suggested that intensive training should come postdoctorally. The conference also favors the inclusion of therapy training in the programs of all graduate students in clinical psychology, but with recognition of the probable fact that not all will be interested in practicing therapy.

Among many suggestions for specific training methods, three papers

present what seem to be both feasible and useful suggestions. Luchins (40) reports that psychotic patients in group psychotherapy were able to give useful criticisms of the trainee therapists who handled the groups. Patients were queried in a group session by the supervisor, and the responses were reported to the trainees in a group discussion. This procedure is recommended as both a training device for group therapy and as a training program in observational methods. Luchins is one of the few who in recent years has attempted to gain information about therapy and therapists by actual querying of the patient. Such attempts as his should provide valuable data on what happens when patients are asked about their reactions to therapy and the therapist.

Haigh & Kell (29) report additional experiences with "multiple therapy" in which two or more therapists deal simultaneously with one patient. Their paper is primarily aimed at emphasizing and illustrating how multiple therapy can aid students and therapists to communicate with one another about their experiences as therapists. All other attempts to understand what happens in a particular session are limited by difficulties in communication. They claim that the postsessions between the therapists who participated promote close scrutiny of what happened, and what was done by each therapist. Excerpts from recorded multiple therapy sessions are presented. It is also suggested that the presence of more than one therapist may provide a research situation wherein various approaches and techniques may be studied on the same patient.

Thompson & Bradway (71) report an intriguing idea they have tried out. Students are assigned prepared roles as "patient" and "therapist," but instead of talking, both persons count aloud or use numbers in any sequence. This procedure is intended to make students sensitive to "the nuances of tone, the emotional interchange, and the expressive movements, as opposed to the words the patient chooses to say." Interestingly enough, both participants and observers report that they know what is happening in the interview. In order to check the preceding statement, a study was conducted in which psychologists tried to match observers' written descriptions of the interviews with wire recordings of the same. All matchings were successful and well beyond chance. The authors also mention that those students more apt at describing what occurs in such interviews also seem to have the higher predicted aptitudes for doing psychotherapy.

Two articles from the psychiatric field stress different aspects of psychiatric training. Murphy & Kligerman (50) testify to the usefulness of recorded interviews in training psychiatric residents in insight psychotherapy. They report that they had very disappointing experiences while attempting to train residents in a Veterans Administration hospital under the usual conditions of tutorial case supervision and clinical conferences. The residents failed to show much progress after six months of this kind of experience. However, when their interviews were recorded, great strides were made as significant interview material, previously unnoted or not reported by the resident, became available for supervisory discussion. It seems that the use

of recording devices, introduced about ten years ago, is by now becoming an almost standard procedure in training for psychotherapy.

Since so little psychotherapy has been practiced with psychotics, as compared with the neurotic or maladjusted, any report on this subject is likely to be of considerable interest. Mann *et al.* (41) report their personal experiences in undertaking psychotherapy with psychotics. The three being psychiatrists, two of whom were simultaneously undergoing psychoanalysis, they were in a favorable position to report thoroughly and frankly their own reactions to the 11 patients with functional psychoses who were treated in individual psychotherapy. Reactivation of the therapist's own conflicts purportedly occurred with considerable frequency when the therapist was dealing with psychotics. As Rosen points out in his discussion of this paper, "Modern psychotherapies seem to be constructed to protect the psychiatrist from any active interpersonal relationship with the schizophrenic patient." [See also Fromm-Reichmann (23) for a more protracted discussion of Rosen's point.]

Finally, in a year when relatively little seems to have appeared on the subject of training for psychotherapy, Rudolph (61) adds a somewhat reassuring note that unconscious thought transmission is likely to occur between therapist and patient only about 5 per cent of the time. However, extrasensory perception (ESP) card tests are advised if therapists become concerned over the possibility of ESP damaging their psychotherapy.

Group Psychotherapy

There is by now little doubt that group psychotherapy, after initially firing the imagination as possibly offering an inexpensive method of treatment, has developed into a new kind of treatment in its own right. Whether this opinion will be held after more results are reported and compared with the individual approach remains to be seen. However, there is considerable evidence that the group approach is adding hitherto unforeseen possibilities to methods of bringing about personality reorganization. Interestingly enough, it is also intensifying the split between two rather vaguely perceived and greatly overlapping schools of thought in the general field of psychotherapy. Traditional psychotherapy, best exemplified by the psychoanalysts, has aimed at releasing repressed material so that the ego would gain greater control over the forces which kept it under pressure from within and robbed it of the necessary energy to deal adequately with the health-producing sexual drives and the demands of external reality. The second school of thought, whose many adherents include such writers as Otto Rank and the American relationship therapists as well as Horney, Sullivan, and their respective followers, has emphasized the importance of changes in interpersonal relationships as the effective ingredient in bringing about improvement. Actually, the two schools cannot be clearly separated on this issue, since the Freudians certainly emphasize and utilize the transference phenomenon as a necessary part of the release process.

In group psychotherapy, the issue seems to be more clear-cut, since it

is rather difficult to emphasize at the same time in a group of four to eight patients both the release of repressed material and the importance of interpersonal relationships. Although anything can be done in work with groups, and apparently almost everything is being tried, group therapists seem about to polarize themselves into those who, like Slavson (64), strive to make group therapy identical with individual therapy, and those others who see the group as a means for stimulating and learning adequate social techniques in a real-life setting.

Slavson's third book on the subject, *Analytic Group Psychotherapy* (64), is an orderly presentation in which the chapters are organized around various principles of therapy, such as transference, catharsis, insight, and so on. Additional concepts, such as universalization, catalysis, dilution, target multiplicity, support, and cathexis displacement, are introduced as a means of describing group interactions which follow psychoanalytic principles. As Slavson says in his preface (64, p. vii).

> Despite this terminological accretion, Analytic Group Psychotherapy is in every *basic* respect identical with individual psychotherapy and dynamic psychiatry. . . . Only to the extent to which this similarity exists can group psychotherapy lay claim to being a valid therapeutic procedure.

The chapters cover with considerable thoroughness most of the important questions arising from group work conducted according to Slavson's point of view. Criteria for selecting groups, the various kinds of analytic group therapy, and work with children, adults, adolescents, and psychotics are discussed; and, except for the one on psychotics, discussions are well illustrated by descriptions and interpretations of actual groups in treatment. Psychotics, Slavson feels, cannot really engage in analytic group therapy, but rather must be dealt with in activity groups. Psychotics need love and attention and do not respond well to more passive attempts on the part of the therapist to stimulate them into forming relationships and revealing underlying dynamics.

Another less thorough but just as systematic presentation of another approach to group psychotherapy is a chapter by Hobbs, entitled "Group-Centered Psychotherapy" (32), which appeared in Rogers' new book (59). The nondirectivists can hardly be said to have been the leaders in developing that form of group therapy which is opposed to Slavson's analytic therapy; Hobbs' chapter, nonetheless, provides one of the more understandable statements of the view of group therapy as providing "an immediate opportunity for discovering new and more satisfying ways of relating to people." For the nondirectivists, of course, the changes produced are basically changes in the self-concept, mediated by new kinds of experiences with people. One is reminded here both of Alexander's "corrective emotional experiences" (1) as necessary elements in treatment aimed at "gradual learning through practice," and Dollard & Miller's (13) explanation of the therapist's providing a situation in which "new conditions permit new learning."

Hobbs is quite definite about the differences between individual and group treatment. The major difference seems to lie in the intriguing notion

that in a group, each patient has the opportunity to become therapist as well as patient. The role of the therapist, however, remains about the same as in the individual approach. The therapist enters actively into the group, as in individual therapy, but with the usual nondirective aims of restating content, clarifying feelings, and indicating acceptance. Members of the group are given priority in taking the role of "therapist." The one difference which appears is that the group therapist strives to reduce threat directed against individual members and to provide opportunities for willing but blocked members to participate.

In reading this account, one wonders about the similarity between the group-centered approach and other approaches which depend upon group relationships. Is there really a basic similarity between these approaches; or are they different in actuality but described in similar words? At the present time, certain words have rather definite positive values among all therapists, words such as "permissiveness" and being "non-tutorial" and "letting the patient gain the insights." Just as Fiedler (20) has shown that some differences as well as some similarities are discernible among individual therapists of different schools, it would appear to be not only interesting but also necessary to have objective comparative studies made of group therapy sessions conducted by different leaders who use the same words in describing their activities. The subtleties of social intercourse to which we usually respond automatically in the social situation are most difficult to describe in definitive terms. The process of understanding any psychotherapeutic approach is inextricably mingled with that of devising words which can be used as unambiguous tools.

Hobbs (32) reports a series of researches by himself and others which seem to throw light on matters of technique rather than those of outcome. Of considerable potential interest is his account of diary-keeping by group members which serves to provide the therapist with additional information on the reactions of the patients to the group sessions. Although application to "outpatients with a psychiatric diagnosis of schizophrenia" is mentioned on page 280 of Hobbs' report, no further account of work with psychotics is found. Other writers have often indicated that group methods, apparently helpful with neurotics, are unsuccessful with psychotics.

The frankest attempt so far to use the patients as therapists in group sessions comes from McCann & Almada (42), who report a somewhat new variant labeled "Round-Table Psychotherapy." Working in a Missouri state hospital, they present a well-detailed account of a method whereby patients on a ward were divided into a round-table group of five or six patients who were being treated, and a "studio audience" composed of the remainder of the ward. The therapist took an unobtrusive role during the half-hour group sessions, although he did prepare the round-table members in advance and sometimes entered into the discussions. The patients were nonviolent and mostly functional psychotics. Since electroshock was also administered, in addition to the group therapy, the authors are reluctant to draw any conclusions as to the effectiveness of treatment, aside from the

statement that it did not seem to hinder readjustment. Some of the novel features of the approach consisted of having the patients themselves make to the medical staff recommendations in respect to discharges and ward transfers. It is implied that the medical staff at first was reluctant to follow these recommendations, but near the end of the 11-month period reported on, the staff seemed to accept the recommendations, and usually acted upon them with approval. The approach seems to parallel to some extent that of Acoholics Anonymous, at least in its explicit attempt to have the mental patients work with one another in simultaneous therapist-patient roles.

A somewhat different interpretation of group psychotherapy appears in a paper by Ezriel (16), a psychoanalyst and member of the Tavistock Clinic in England. Ezriel describes, as part of his interest in psychoanalysis as an ahistorical, nongenetic approach (17), a group therapy technique based upon rigorous transference interpretations confined to the "here and now." The therapist continuously guides his interpretations by constantly trying to answer for himself the question: "What makes these people say these things at this moment?" He believes that when several people meet in a group, "each member projects his unconscious phantasy-objects upon various other group members and then tries to manipulate them accordingly." The therapist's task becomes one of bringing the immediate transferences into the open.

Ezriel also gives a brief but thoughtful summary of his impressions of group therapy, based on four years' experience with it. He suspects that some cases show greater gains under group therapy than they would have in a comparable period of individual psychoanalysis. Other cases, however, fail to show much improvement under the group conditions.

The expected array of reports on the application of group methods to different clinical problems appears. New twists are still offered with considerable frequency. Baruch & Miller (4) have introduced drawings prepared by individual members in advance of the sessions, and the drawings seem to serve as a stimulus for release in both the "drawer" and the other members of the group. The amazingly frank discussions of individual attitudes and experiences leaves little doubt that carefully-organized group treatment will result in the production of exceedingly intimate sexual material, even in nonhospitalized adults.

Stone & Levine (66) present further evidence that the most intimate material will appear in group discussions, although they worked with husbands and wives in separate groups. At the Margaret Sanger Research Bureau in New York, they used a combination lecture-discussion method to work out minor sexual maladjustments, such as failure to reach a climax in intercourse. The authors point out that this approach provides a possibly fruitful method for research in sexual attitudes and sexual experiences.

Another possible advantage for the group approach is reported by Randall & Rogers (56), who treated outpatient epileptics with a combined lecture-discussion approach. Among their patients were a large number of Spanish-Americans, with whom relationships are difficult to establish in an

individual approach; but in the group approach, there was much greater spontaneity and participation by them.

Caplan (8) also comments on what he assumes to be the greater willingness of people to speak more readily about their intimate and private feelings in a group than when they are alone with a therapist. The whole matter of determining the optimum conditions for obtaining self-revelations certainly cannot be decided by a simple studying comparing group with individual sessions. Factors of group organization, personalities of the participants, socio-cultural and racial backgrounds, and personalities of the therapists provide many obstacles to any research pointed in this direction. Nonetheless, work to date seems to indicate that under certain conditions people will reveal themselves more readily in group sessions than in individual sessions. What are the necessary conditions?

Among other problems, the homogeneity of group members is still one of the more perplexing. Almost every writer presents his ideas on this topic, but still there seems to be little uniformity. The problem is evidently similar to the one just discussed concerning frankness of revelation in a group, versus an individual setting. There are many more factors in a group situation than the highly artificial concept of "homogeneity of diagnosis" would indicate. Taylor (70), for example, thinks that mixed groups of male and female neurotics are highly beneficial to group therapy.

Most group psychotherapy, except that of Moreno and his followers, has attempted to duplicate the permissive, nontutorial atmosphere of the individual approach. (The lecture-discussion-education approach is not under discussion here.) Luchins (40), however, reports a highly aggressive and direct attempt to demonstrate to male outpatients in a Veteran's Administration clinic the opinions which others have of them. This was accomplished gradually over a period of several months by such procedures as (*a*) playback and discussion of a portion of a previously recorded group therapy session, (*b*) group discussion of a previous session of the same group, and (*c*) asking members of the group to report, while in the group, their impressions of other members. Luchins indicates that the results, based largely upon clinical impressions, were not particularly disruptive to the group or to individuals. The effect upon therapeutic progress, however, could not be determined. An able rebuttal of the expected contention that such procedures are too intellectual and not sufficiently emotional is also presented. As an attempt to alter the patient's perception of himself by other than currently fashionable methods, the procedures here utilized seem worthy of further investigation.

Only two articles, both from the same author, which dealt exclusively with the training of the group therapist were found. Grotjahn (27), discusses the counter-transferences of the group therapist, and hopes that an analysis of the therapist may lead to an understanding of the dynamics of the group. Furthermore, he suggests (28) that progress in a group can often be increased if the supervisor of the group therapist adds occasional participation in the group to his supervisory discussions.

Nondirective Psychotherapy

The big event of the year in the nondirective field has undoubtedly been the publication of Rogers' *Client-Centered Therapy* (59). The nondirectivists have pursued objective research more assiduously than any of the other schools, and the new book serves as a good compilation of almost all of the research work to date. How much the research has actually influenced the somewhat altered presentation of the nondirective approach is, of course, an open question. Since, however, the by-products of research—such as close questioning of one's procedures and forcing oneself to adopt varying points of view in order to examine closely one's own hypotheses—are often as effective as contemplation of the actual results obtained, there is little doubt that objective research has had its effects. One still gains the impression that the nondirective research to date has failed to deal with the more crucial issues in psychotherapy, such as the isolation of the factors which are basic to personality change and the providing of objective data on the actual changes which take place as a result of treatment; but these are problems which all approaches have in common and which cannot be laid at the door of one school only. Current researches, sponsored by the Rockefeller Foundation and now underway at the University of Chicago Counseling Center, give promise of attacking each of these major issues at a more profound level than any objective research to date. It will probably be another year or two, however, before the results will be available for publication.

The new emphasis on technique in *Client-Centered Therapy* appears to be a logical outcome of the early direction pursued by the nondirectivists, plus the injection of a new theoretical foundation acquired since the publication, ten years ago, of *Counseling and Psychotherapy* (58). The new method now calls for an abandonment of techniques as ends in themselves, and concentrates upon adopting the client's frame of reference, perceiving with him in an atmosphere of warmth, acceptance, and respect. In Rogers' own words (59, p. 34),

> it is the counselor's aim to perceive as sensitively and accurately as possible all of the perceptual field as it is being experienced by the client, with the same figure and ground relationships, to the full degree that the client is willing to communicate that perceptual field; and having thus perceived this internal frame of reference of the other as completely as possible, to indicate to the client the extent to which he is seeing through the client's eyes.

Although this is somewhat reminiscent of the analyst's attempt to allow his own unconscious full reign in "communicating with the unconscious of the analysand," the two approaches differ in their aim. Whereas the analyst hopes to gain additional knowledge of the dynamic forces in order to interpret them to the patient, the nondirectivist is utilizing the "deep understanding" as a means of providing the client with "a most meaningful expression of basic confidence in the forward-moving tendencies in the human organism" (59, p. 36). Furthermore, the experience of being completely understood and accepted by another person permits the client to

perceive, objectify, and therefore control through reorganization the basic aspects of himself. Although the concept of the "unconscious" is alluded to only twice, frequent references are made to experiences and attitudes which have been "denied to awareness" in much the same fashion as Fenichel (18) employs the term "warded off." Thus, the process of total understanding seems to involve the therapist helping the client to perceive all the relevant factors in himself so that he can become aware of them and thereby learn to reorganize them. This is not, of course, a simple insight theory nor a simple uncovering theory. It is an approach in which the relationship is utilized directly as a powerful tool for strengthening the client's self-confidence so that he may proceed to reorganize his own personality.

Stated in those terms, the basic essentials of the approach seem to be very similar to those of some of the Neo-Freudians, such as Alexander, Horney, or Sullivan, who aim at strengthening the ego by means of transference relationships so that new, and presumably more healthful or integrating, experiences will occur. Whether the similarity is actual, or perceived only as an idiosyncrasy of the reviewer, is, of course, a question for others to decide. This is not to imply that the techniques used by these various approaches are necessarily identical, although from Fiedler's study (19, 20) one gains the impression that they may have much in common. Again one wishes that there were methods available for describing techniques in terms of their effects upon the client rather than (as is the only possible way at present) describing what the therapist thinks he is doing. Furthermore, in a complex organization of events, there is no reason why different organizations of somewhat differing techniques might not achieve the same goal.

Writing persuasively as always, Rogers includes frequent illustrations of his major points by drawing freely upon verbatim excerpts from therapeutic interviews. In addition to separate chapters written by colleagues on play therapy (Elaine Dorfman), group-centered psychotherapy (Nicholas Hobbs), and group-centered leadership and administration (Thomas Gordon), he also presents a 55-page "Theory of Personality and Behavior." The theory is presented in the form of 19 propositions, and can best be summarized by a quotation from Rogers (59, p. 532):

> This theory is basically phenomenological in character, and relies heavily upon the concept of the self as an explanatory construct. It pictures the end-point of personality development as being a basic congruence between the phenomenal field of experience and the conceptual structure of the self—a situation which, if achieved, would represent freedom from internal strain and anxiety, and freedom from potential strain; which would represent the maximum of realistically oriented adaptation; which would mean the establishment of an individualized value system.

A second book by one of the nondirectivists introduces something new and different into the field of psychotherapy. Porter's *An Introduction to Therapeutic Counseling* (52) is essentially a workbook for students interested in counseling and psychotherapy. Unlike the usual workbook, which is primarily intended as a small companion for a more comprehensive text,

Porter has taken some of the basic problems in technique, analyzed them into their basic essentials in front of the reader, and then provided a series of practical examples of each problem which the student can work out in his own fashion. Although Porter's own orientation is obviously that of the nondirectivist, the book is pitched at a level where problems in technique can be analyzed within the theoretical framework of any school. The goal is not necessarily to train the student in nondirective techniques (although it would be surprising if that did not occur in most instances), but rather to provide material whereby the student can recognize technique problems as they occur and then decide, within his own frame of reference, what kind of solution is required.

Porter's book is likely to become a standard part of the therapeutic training program for beginning students in many psychology departments. Psychotherapists are usually the first to point out that one cannot learn how to do psychotherapy by reading about it or by listening to someone talk about it. That statement probably still holds true, but this book should provide an intermediate step, whereby someone who has had a few experiences in dealing with the foibles of clients and patients can practice exercises which should make him much more sensitive to the realities that he has had so little opportunity to perceive. The book will not make competent therapists, but it will help students to deal more quickly with the relevant variables in therapy as we understand them at the present time.

Only a few articles published by the group of nondirectivists have appeared during the past year, and there seems to have been no really critical attack against it by members of other schools of thought. Wood (78) questions whether transference does not occur in the client-centered approach, but settles the problem on a reasonable note by asking that nondirective interviews be studied to find out whether transference relationships do or do not occur. This argument seems still to be confused by the failure to separate the notion of emotional attachment from the idea of repetition of characteristic behavior (or parent-centered behavior) on the part of the patient. Thorne's new book (72) makes a number of highly complimentary remarks about Rogers' approach, but suggests that it is useful largely for dealing with normals or those emotionally blocked (maladjusted).

Early in the year, Cowen & Combs (11) published one of the few follow-up studies in the literature of psychotherapy. As their work was concerned mainly with college counselees, it will be mentioned here only as a good example of the difficulties involved in attempting to do follow-up. Blau (5) reported a tantalizing fragment of a research which indicated that analysis of self-references during the first interview may be predictive of the eventual success or failure of persons treated by the nondirective approach. Only seven cases were analyzed, but the predictions agreed highly with the rated outcomes.

Directive Psychotherapy

In another new and important book, Thorne, the author, writes (72, p. 86):

As a reaction to the uncritical acceptance of the nondirective viewpoint, Thorne introduced the term "directive" to emphasize the existence and nature of other methods utilizing more active direction on the part of the counselor.

Since only a few other writers have adopted the term "directive psychotherapy," and then only sporadically, it would appear that the term applies largely to Thorne's own work. His book, however, should not be viewed as only a reaction against the nondirectivists, but rather as an attempt to summarize in an eclectic fashion what one clinical psychologist, who is also a medical doctor, has found to be currently useful in the various psychotherapeutic approaches. Thorne has taken literally the generally accepted statement that all methods produce successes and that all methods of therapy seem to produce a reasonable proportion of failures. Thus he surveys the currently-advocated approaches in a complex but integrated framework of eclecticism. As Professor William A. Hunt writes in the foreword,

Despite the designation "directive," which Dr. Thorne attaches to his formulation of the principles of therapeutic counseling, his book is essentially eclectic in that it represents the application of all pertinent knowledge of psychology to the problem. There is no exclusion of material for doctrinaire purposes.

Thorne's eclecticism is both a weakness and a strength. His book will probably be read less widely than it deserves because most readers in this field are as partisan as the writers, and a book on psychotherapy which fails to center its attention on one central theme is likely to be cast aside as contributing very little. The strength of his book lies in the fact that while organized like a medical text or reference, it is much better than most such works found in the field of psychiatry, because Thorne shows a much more thorough comprehension of underlying principles and concepts of the various approaches than is usually found in the publications of over-worked and practice-centered psychiatrists.

Many persons will quarrel with Thorne over many items: both those included in his book and those omitted. The same would be said about any writer who attempted to summarize not only the theoretical notions of present-day psychotherapists but also their techniques. In a sense the book is overly ambitious in conception. There is a serious question as to whether the field of psychotherapy is ready for a truly eclectic approach, since so many of its notions are based upon individual fancies rather than facts. Furthermore, the introductory chapters on the scientific basis for personality counseling, several of which were published earlier in the *Journal of Clinical Psychology*, although making excellent background reading for those interested in the probable future of psychotherapy, have little relevance to the work actually being done today.

Psychoanalysis

It would be imprudent for one without a thorough background in psychoanalysis to enter into the family discussions and to attempt a critical survey of a year's output in a field so complex in its theorizing and so remote from the concepts and terminology of American psychologists as is psychoanalysis.

XUM

Psychoanalysts have devised a culture of their own in which violent upheavals appear largely in the form of groundswells, and new ideas are hard to identify as such because of the likelihood that, to be facetious, "it was what Freud would have said had he continued his paragraph."

This section will not, therefore, attempt the usual survey, but instead will merely point to what has struck the reviewer's fancy. To preface the discussion, it should be pointed out that the past decade, following the death of Freud, has seen the growth of numerous Neo-Freudian revolts as a major trend in psychoanalysis, with both the orthodox and the revisionists paying particular attention to ego-psychology. As usual, multiple causation is probably more likely than the single cause; and, in the case of ego-psychology, Freud himself had much to do with its increasing popularity.

Perhaps typical of the increasing tendency to revise Freud both in theory and therapeutic technique is the book of Fromm-Reichmann, *Intensive Psychotherapy* (23). This author, apparently strongly influenced by Sullivan, after initial orientation by Freud and Groddeck, has written a very readable book which attempts a synthesis of Freudianism and Sullivanism. The latter seems to have won some important victories in both theory and technique.

Fromm-Reichmann not only advocates the chair rather than the couch, but also interpretive discussion rather than free-association. She evidently agrees also with the principle of "flexibility" as sponsored by the Chicago Institute for Psychoanalysis, and tends to play down the need for daily sessions. An analyst would probably detect many more heresies in her technique than this reviewer. Her theoretical heresies are similar to those of Sullivan and Horney. The broader concept of "parataxic reactions" is substituted in some degree for "transference reactions." The defenses are seen as "security operations." The repetition-compulsion is somewhat deemphasized in favor of the reality of interpersonal relationships

Her greatest deviation seems, however, to lie in her cavalier treatment of the Oedipus complex about which, she says, (p. 6),

> Our thinking does not coincide with Freud's doctrine of the ubiquity of the Oedipus complex . . . . Consequently, we do not understand as a foregone conclusion that the difficulties of therapists in their relationships with patients and vice versa stem from, or are only a repetition of, their unresolved Oedipus constellations.

And again, (p. 99),

> In my experience the wish for closeness and tenderness with the beloved parent and the envious resentment about the authoritative power of the hated one, both without recognizable sexual roots, constitute a more frequent finding in childhood histories of healthy, neurotic, and psychotic people than do their sexual Oedipal entanglements with the parents of their childhood.

The revolution is complete when she reverses the significance of the sex act and, instead of genital maturity producing mental health, she says that a person with good interpersonal relationships is now capable of a normal sex life (see p. 35).

Fromm-Reichmann's previous publications might lead one to hope that she would have written primarily about treatment of psychotics. This she does to some extent, but she still leaves the reader with a feeling that she knows much more about the intimate reactions of psychotics than she divulges in this book. In regard to Rosen's (60) provocative reports on direct interpretation of schizophrenic reactions, Fromm-Reichmann shows considerable uncertainty. In one footnote (23, p. 95) she decides that her technique is quite different from Rosen's, while in another (p. 180) the difference becomes a matter of degree in that

> Rosen addresses himself more to the infant and the child in the psychotic. The method advocated here is based on stressing psychotherapeutic contact with the patient as a person of his present age level.

Perhaps the greatest value of the book, however, is the profusion of psychotherapeutic insights into the therapist-patient relationship. On almost every page, the author presents one or more practical suggestions in regard to handling patients of particular categories, what and how to interpret, ingenious illustrations of therapeutic technique, and telling comments on counter-transference difficulties. This last item needs, perhaps, special mention. There is little doubt that lack of an objective observer in individual therapeutic sessions has hampered the development of an understanding of therapist error. Fromm-Reichmann spends the first 45 pages detailing desirable attitudes and frequent mistakes on the part of the psychotherapist. In no uncertain terms, she hammers away at the many instances in which the therapist, out of ignorance or because of his own needs, is likely to wound the self-esteem of the patient. To avoid humiliation of the patient is for her (and Sullivan) a major goal of the therapist. She is particularly effective in pointing out the subtle and often socially-acceptable ways in which therapists have been prone to humiliate the patient. Like all fluent practicing analysts, Fromm-Reichmann writes about psychotherapy with a richness of insight which makes the reading of her somewhat unsystematic book very rewarding.

A second book by an analyst, Erikson's *Childhood and Society* (15), is not easy reading, and in many ways baffles rather than clarifies. Erikson writes more like a poet than a psychologist, although there is little doubt that he is thoroughly conversant with psychoanalysis, child psychology, and Gesell's variety of developmental psychology. Since Erikson is dealing largely with analysis of children only brief reference to his book will be made here. It is cited particularly for his attempt to delineate a somewhat novel relationship between the standard psychoanalytic theory of erogenous zones and the developing child's interaction with his society. In many ways the approach is reminiscent of both French's (22) field approach and of Cameron's (7) explanation of the functional significance of body apertures as vehicles for learning "visceral ethics." Erickson, however, recasts the Freudian theory of zonal development, while still retaining the basic principle of predetermined, normal sequences. The case studies of children he has treated are almost too engaging to be read critically. Certainly they cannot

XUM

be understood without letting one's unconscious wander as freely as does the author's, and trying to wander with him is no mean trick.

Only a few of the many articles on psychoanalysis will be mentioned. Jokl (34) presents a spirited defense of classical analysis, aimed largely at those who would dispense with free association. The "basic rule," says Jokl, is the only sure technique for permitting psychic determinism to operate effectively in the analytic session. Sterba (65) reports some historical research on Freud's "cure" of an arm affliction of a well-known orchestra conductor. Sterba claims, therefore, that Freud employed brief psychotherapy.

On the question of interpretation, Loewenstein (37) has a simply written summary of the main aspects of interpretation as viewed by the analyst. Kris (36) discusses new approaches to interpretation, stemming from the newer emphases on ego-psychology. For some patients at least, he advises against "direct or rapid access to the id through interpretation." Instead, there should be an initial exploratory period, then descriptive interpretations, which gradually try to establish typical patterns of behavior past and present. Kris is cited here mainly to point out the increasing interest shown by analysts in the so-called "artificial" aspects of their patients' lives, in contrast to earlier emphasis on the "depth" factors.

Ezriel's interesting paper (17) presents an attempt at integration and a suggestion which might well be of interest to research-minded psychologists. He says that the recent upsurge of interest in transference reactions means that "Psychoanalysis has thus become a method of studying the personality of an individual through investigating his interactions with another person." Thus, psychoanalysis becomes an ahistorical and dynamic, not a genetic, method. While, he continues, psychoanalysis is not capable of yielding any conclusive evidence of a patient's past (because of the patient's inability to distinguish between reality and fantasy), the transference technique "enables us to make use of all the material of a session as evidence of what determined the patient's behavior in that session." Thus he suggests that analysis of transference reactions can be studied and predictions made. This may not be psychoanalysis, but it does sound like a possible method for studying transference, even in a laboratory setting.

Alexander (1) presents a lucid and reasonable exposition of his developing views on the place of the "corrective emotional experience" in psychotherapy. Considerable emphasis is placed upon the relationship between patient and therapist as a means of producing the necessary corrective emotional experiences, which are essential parts of treatment characterized as a "gradual learning through practice." This article is an even more readable account of the Chicago Institute's re-examination of the therapeutic process than the 1946 *Psychoanalytic Therapy* (2) by Alexander & French.

For those interested in the outcome of any method of treatment it may be said that Oberndorf (51) bewails the situation and asks, with gentle chiding, for more research into the causes of analytic failures. He suggests that a questionnaire might be sent to analysts and to their patients, although

he would prefer to have unsatisfactory "outcomes" studied by the analyst in conference with his colleagues. He complains that over the past ten years he has found practically no literature dealing with the results of psychoanalytic treatment. As a former editor of a psychoanalytic journal, Oberndorf has a good vantage point to attest to the unwillingness, or inability, of most psychotherapists to present reports of results of treatment.

Learning and Psychotherapy

As indicated in last year's review, a number of rather important articles appeared dealing with attempts to organize the process of psychotherapy in terms of learning theory. During the past year, only a few articles on this topic appeared; all of these are greatly overshadowed by Dollard & Miller's book *Personality and Psychotherapy* (13). It is suspected that the formulation of psychoanalytic therapy in Dollard & Miller's book will have a number of important effects for several years to come. In the first place, graduate students in clinical psychology will probably find an intimate knowledge of this book very useful in their final, comprehensive examinations. Secondly, one can expect a "rash" of new research to be aimed directly at the clearly-stated principles which these authors espouse. Adequate research designs that deal directly with patients undergoing treatment will, as always, be developed by only a few and with the greatest of difficulty. However, much research will probably be planned around normal individuals in the hope that the variables studied will have application to psychotherapy itself. Perhaps Hilgard's dream of a laboratory of psychodynamics (31) will come a step closer to reality.

Interest in explaining the phenomena of psychotherapy in terms of learning theory is not new: it certainly dates back, at least as far as explicit statement is concerned, to the Muenzinger article (48) of 22 years ago. Since that time, most psychologists interested in the field have sooner or later and sometimes reluctantly come to a conclusion that psychotherapy is an applied learning process. Such an orientation has not as yet (and this includes the book now under discussion) added anything significant to the practice of psychotherapy, except perhaps the use of aversion methods in reconditioning alcoholics. By and large, however, the orientation toward learning theory presents a hope that sometime in the future a realistic learning theory will provide a basic set of principles which will explain and predict the events necessary and sufficient to bring about induced changes in personality.

Dollard & Miller's book (13) certainly is a step in this direction, although whether the step will have to be retraced and a new direction mapped out cannot be predicted on the basis of the present evidence. Essentially, the book accepts the psychoanalytic notion of the relevant variables in the analytic process but organizes them by reinterpretation into a learning framework. The learning theory is basically that of Hull, but a beautifully nontechnical Hull. In fact, Part II, "Basic Principles of Learning," is as clear-cut and readable a statement of the general Hullian approach as one

can hope to find. Even so, quite probably it will be very "hard going" for persons in other disciplines who are already interested in psychotherapy as learning but have no technical background in the latter area.

Dollard & Miller's basic concepts from psychoanalysis are relatively orthodox. Sex and aggression are accepted as the major motives of human beings, although other drives are also recognized—particularly anxiety. The Oedipus complex and its resolution are satisfactorily explained in terms of castration anxiety, and "the Unconscious" is still quite recognizable despite its also being labeled the "unverbalized."

Adopting the standard categories of drives, cues, responses, and reinforcement as the basic factors in learning, the authors do a very capable job of showing how most, if not all phenomena turned up by psychoanalysis might be "explained" in terms of learning principles. Repression, for example, "is explained as the inhibition of the cue-producing responses which mediate thinking and reasoning" and "Transference is seen as a special case of a wider concept, generalization" (13, p. 10). The well-known difficulties encountered in treating alcoholics, drug addicts, and the sexually abnormal are very neatly explained in terms of reinforcement theory. "The strong immediate reinforcement from relatively rapid drive reduction is another reason why drug addictions and perversions are difficult to treat" (p. 234).

While the whole book is eminently readable, the second half, which deals with new conditions of therapeutic learning, conflict, and special aspects of therapy, presents one of the best descriptions in the literature of the most generally accepted techniques, admonitions, and rules of thumb found in the practice of almost all psychotherapists. Even without the first half of the book, which deals with principles of learning, the second half could almost be used alone as an introduction to the principles of psychotherapy. Despite its definite psychoanalytic orientation, probably few psychotherapists will quarrel much with the general contents.

The basic question of how well Dollard & Miller have explained psychotherapy as a learning phenomenon cannot be dealt with at the present time. Only facts can answer such a question, and the facts must, of course, be drawn from direct study of the phenomena under examination. The facts of Dollard & Miller are largely "second order" in that they deal with learning studies of rats and of people who are not undergoing psychotherapy. In addition, they draw on data reported by psychoanalysts; some of these data are more or less confirmed, while the rest are merely part of the trappings of psychoanalytic culture.

From a theoretical standpoint, the major difficulty one encounters in accepting Dollard & Miller's very competent analysis lies in the drive-reinforcement area. Granting the acceptability of the reinforcement hypothesis one still wonders at the ease with which drives are identified. To "burlesque" without rancor (since all psychologists are faced with the same problem), one merely postulates the appropriate drive and all problems immediately become simple. Appropriate drives can then be found any place and any time—before, after, in cues, or in responses. In other words,

the theory cannot fail to work as long as drives can be identified without fear of contradiction.

While drives can be artificially induced with some certainty in the laboratory, although even here the studies on latent learning have encountered difficulty, trying to identify drives in the psychotherapist's office is quite a different matter. The old question of "what is this patient really trying to do?" must be answered before one can tell with much certainty what responses to reinforce. And if the therapist cannot identify the actual drive or drives he is dealing with, he is hard put to know what to do. The nondirectivists have, of course, solved this difficulty in the practical sphere by saying, "Let him find his own drives. Let him do his own reinforcing."

The journals have yielded relatively little that is new in regard to this field. Torrance suggests (74) that the so-called "growth principle" is best explained in terms of *Prägnanz* (Wertheimer). Therapy, a learning matter, follows the principle of *Prägnanz* and can be best understood in such a framework.

One study dealing with stutterers was specifically designed to examine stuttering as a product of learning in the hope that learning principles would also point to the appropriate therapy. Sheehan (63) approached stuttering with the assumption that stuttering is reinforced because the stuttered word, despite the distortion in speech, reaches the goal of completing the speech act, and thus the drive for that word is reduced. This hypothesis suggests that if nonreinforcement of stuttering can be obtained, reduction in stuttering should occur. Twenty stutterers, who served as their own controls, read standard passages aloud under two sets of instructions. When the instructions set the goal of correct pronunciation, theoretically stuttering should not be reinforced. The second set of instructions merely asked the subjects to read the standard passages. Sheehan found statistically significant differences between the two conditions of speaking; there was less stuttering when the instructions set correct pronunciation as the goal. As Sheehan points out, this study apparently contradicts the frequent assertion that parents should avoid making their child-stutterer "say it over and say it right." He suspects that such home treatment often worked when the child accepted the instructions and did not regard them as punishment.

Hypnosis and Other Adjuncts

Hypnosis has long fascinated psychotherapists because of the ease with which short-term changes in personality can be accomplished under the influence of hypnotic suggestion. In addition, the ease with which buried memories can often be tapped under hypnosis provides hope that catharsis might be accomplished directly and efficiently and without the long-time probing and associating usually demanded by nonhypnotic techniques. These hopes rest, of course, upon the unproved assumption that catharsis is an essential factor in the redirecting of behavior.

Recent years have seen a decided upsurge of interest in hypnosis as a therapeutic technique. The naive belief that hypnosis, coupled with direct suggestion, would accomplish miracles has practically disappeared from the

literature. Emphasis over the past decade has rested largely upon attempts to determine how hypnosis might be used as a psychotherapeutic adjunct. Approaches utilizing hypnosis in conjunction with some form of analysis have reached an uncertain stage of respectability under the name of "hypnoanalysis." No particularly new insights into hypnosis as a phenomenon or as an applied technique have appeared recently, although many workers seem active in the investigation of this psychological mystery. A book by Heron, entitled *Clinical Applications of Suggestion and Hypnosis* (30), for the most part deals with hypnotic analgesia in dentistry, obstetrics, and the other medical fields, and makes only passing reference to hypnosis in psychotherapy.

Gill (24) specifically denies that hypnosis necessarily obliterates defenses, and discusses various possibilities for making use of hypnosis as a much broader tool than it is ordinarily conceived to be.

Although Moreno had suggested "hypnodrama" several years ago, his first descriptive publication did not appear until last year (45); in this he predicts that (now that the therapeutic theater has surpassed psychoanalysis) "by revolutionizing the *vehicle form* and *concept* of treatment," hypnodrama will be found to be the essence of all attempts to use hypnosis therapeutically.

> The hypnotic operation is in itself of limited value, it can gain new momentum as a psychological starter to psychodramatic production. It can serve a similar purpose as the psychochemical starters, insulin, electric shock, alcohol, etc., and replace them in many instances.

One of his followers, Enneis, adds substance to Moreno's introductory comments by a practical description of hypnodrama (14). He says, "While the more profound spontaneity states may be achieved without hypnosis, in many cases it requires long and arduous training. With hypnosis it is achieved more easily." Thus, hypnosis in hypnodrama appears to be only a means for obtaining more rapid co-operation from the patient, after which psychodrama is carried out as usual. Enneis includes two apparently recorded sessions of hypnodrama in his paper.

As we draw further away from the notion that catharsis is the basis for successful psychotherapy, it may be that hypnosis will find a more useful role in psychotherapy.

A number of other adjuncts to psychotherapy have been suggested recently by various authors. Whitaker *et al.* (75) experimented with the possibility of bringing a consulting therapist into individual psychotherapy when treatment reached, as it often does, an apparently insoluble impasse. The presence of the consultant has, they claim, worked with surprising effectiveness in some instances. They also suggest less radical methods for dealing with "stalemates," such as confidential consultation with a colleague or open discussion of the impasse with the patient, interest being centered on the defects of the therapeutic relationship.

Wilcox (77) describes a new method of therapy called "psychopenetration," in which five inhalations of 20 per cent carbon dioxide are administered as a means of tapping the unconscious. A series of 60 special questions

has been devised to test the possibility of "reaching the unconscious" following carbon dioxide inhalations. If unsuccessful, the patient is not considered treatable by psychotherapy. No data are given, although training in the technique for diagnosis and treatment is available and under way in a state hospital. Cohn (9) provides a brief report of an informal study of the effects of drugs (caffeine, benzedrine, and the barbiturates) on the behavior of patients in group therapy over an eight-week period. Although no control groups were used, "both benzedrine and the barbiturates increased social interaction and stimulated the discussions." The history of drugs as therapeutic adjuvants has somewhat paralleled the history of hypnosis in the field of psychotherapy, with approximately the same disappointing results.

One new adjunct, "film therapy," is proposed by Prados (53) as a means of "shaking up the individual and group defenses" in group therapy. He compares the film experience with dream experiences and claims that after each film the group discussion provides much more opportunity for dealing with defenses directly.

Increasing attention is being paid to "so called" chronic patients formerly neglected almost entirely. Chronic patients receiving only custodial care make up a large proportion of persons occupying beds in mental hospitals. As the concept of "deterioration" as an irreversible process where the functional psychoses are concerned becomes increasingly dubious, some hope is being felt that chronic patients may eventually be helped. Mott (47) reported a dramatic recovery of a 72-year-old woman hospitalized for 17 years and regarded as a hopeless, chronic, and deteriorated schizophrenic. Apparently complete recovery was achieved by rather infrequent psychiatric interviews over a six-month period. This case report, presented pictorially in a recent issue of *Life* magazine, has aroused some public as well as professional interest although the phenomenon was not unknown in previous psychiatric history. Timm & Davis (73) present one of the more thoughtful discussions concerning the aims and methods of an activity (nonverbal) approach to dealing with chronic patients in mental hospitals. They hope that by proper selection of activities in terms of form, content, and intensity, the therapist may provide "levels of expression to carry out more specific therapeutic aims." Such reports as this indicate that the provocative but "shotgun" approach of the "total push" may some day be systematically analyzed into a controllable system for dealing with hospital patients on a rational, rather than "evangelical" basis.

Psychotherapeutic adjuncts have appeared in practically all possible forms during the development of modern psychotherapy. The problem of creating adjuncts seems to be much simpler than that of deciding what are the effective factors in psychotherapy. It would appear that until the central issues can be settled effectively, the usefulness of adjuncts must remain undetermined—unless, by happy chance, a particular adjunct turns out to contain all the necessary and indispensable factors. The recent history of the shock and coma therapies offers interesting parallels to most of the other adjuncts which have been tried.

XUM

LITERATURE CITED

1. Alexander, F., *Psychoanal. Quart.*, **19,** 482–500 (1950)
2. Alexander, F., and French, T. M., *Psychoanalytic Therapy* (The Ronald Press Co., New York, 353 pp., 1946)
3. Barry, J. R., *J. Abnormal Social Psychol.*, **45,** 647–58 (1950)
4. Baruch, D. W., and Miller, H., *Am. J. Psychother.*, **5,** 45–49 (1951)
5. Blau, T. H., *J. Clin. Psychol.*, **6,** 403–5 (1950)
6. Bordin, E. S., *J. Clin. Psychol.*, **4,** 240–44 (1948)
7. Cameron, N., *The Psychology of Behavior Disorders* (Houghton Mifflin Co., New York, 622 pp., 1947)
8. Caplan, G., *Mental Hyg.*, **35,** 41–51 (1951)
9. Cohn, C. H., *Am. J. Psychiat.*, **107,** 135–36 (1950)
10. Collier, R. M., *J. Consulting Psychol.*, **14,** 199–205 (1950)
11. Cowen, E. L., and Combs, A. W., *J. Abnormal Social Psychol.*, **45,** 232–58 (1950)
12. Dengrove, E., and Kutash, S. B., *Am. J. Psychother.*, **4,** 457–73 (1950)
13. Dollard, J., and Miller, N. E., *Personality and Psychotherapy* (McGraw-Hill Book Co., Inc., New York, 488 pp., 1950)
14. Enneis, J. M., *Group Psychother.*, **3,** 11–55 (1950)
15. Erikson, E. H., *Childhood and Society* (W. W. Norton & Co., Inc., New York, 397 pp., 1950)
16. Ezriel, H., *Brit. J. Med. Psychol.*, **23,** 59–75 (1950)
17. Ezriel, H., *Brit. J. Med. Psychol.*, **24,** 30–4 (1951)
18. Fenichel, O., *The Psychoanalytic Theory of Neuroses* (W. W. Norton & Co. Inc., New York, 703 pp., 1945)
19. Fiedler, F. E., *J. Consulting Psychol.*, **14,** 239–45 (1950)
20. Fiedler, F. E., *J. Consulting Psychol.*, **14,** 436–45 (1950)
21. Fiedler, F. E., *J. Consulting Psychol.*, **15,** 32–38 (1951)
22. French, T. M., *Psychoanalytic Quart.*, **11,** 48–55 (1942)
23. Fromm-Reichmann, F., *Principles of Intensive Psychotherapy* (Univ. Chicago Press, Chicago, 246 pp., 1950)
24. Gill, M. M., *Psychoanal. Quart.*, **20,** 62–72 (1951)
25. Goldston, I., *et al.* (Symposium), *Am. J. Psychother.*, **4,** 419–57 (1950)
26. Gottlieb, J. S., and Huston, P. E., *J. Nervous Mental Diseases*, **113,** 237–47 (1951)
27. Grotjahn, M., *Psychiatry*, **13,** 63–68 (1950)
28. Grotjahn M., *Group Psychother.*, **3,** 309–15 (1951)
29. Haigh, G., and Kell, B. L., *J. Abnormal Social Psychol.*, **45,** 659–66 (1950)
30. Heron, W. T., *Clinical Applications of Suggestion and Hypnosis* (Charles C Thomas, Publisher, Springfield, Ill., 116 pp., 1950)
31. Hilgard, E. R., *Am. Psychologist*, **4,** 374–82 (1949)
32. Hobbs, N., "Group-Centered Psychotherapy," in *Client-Centered Therapy*, 278–319 (Rogers, C. R., Ed., Houghton Mifflin Co., New York, 560 pp., 1951)
33. Hunt, J. M., *J. Consulting Psychol.*, **13,** 69–81 (1949)
34. Jokl, R. H., *Bull. Menninger Clinic*, **14,** 207–20 (1950)
35. Kaufman, P., *J. Consulting Psychol.*, **14,** 458–64 (1950)
36. Kris, E., *Psychoanal. Quart.*, **20,** 15–31 (1951)
37. Loewenstein, R. M., *Psychoanal. Quart.*, **20,** 1–14 (1951)
38. Lord, E., *J. Consulting Psychol.*, **14,** 134–39 (1950)
39. Luchins, A. S., *J. Consulting Psychol.*, **14,** 446–51 (1950)
40. Luchins, A. S., *J. Consulting Psychol.*, **15,** 24–31 (1951)
41. Mann, J., Menzer, D., and Standish, C., *Psychiatry*, **13,** 17–23 (1950)
42. McCann, W., and Almada, A. A., *J. Consulting Psychol.*, **14,** 421–35 (1950)

XUM

43. Mensh, I. N., and Watson, R. I., *J. Clin. Psychol.*, **6**, 237–42 (1950)
44. Mensh, I. N., and Golden, J. M., *J. Clin. Psychopathol.*, **11**, 178–89 (1950)
45. Moreno, J. L., *Group Psychother.*, **3**, 1–11 (1950)
46. Morton, R. B., *Am. Psychologist*, **4**, 272–73 (1949)
47. Mott, J. M., *Bull. Menninger Clinic*, **14**, 177–83 (1950)
48. Muenzinger, K. F., and Muenzinger, F. W., *Mental Hyg.*, **13**, 250–62 (1929)
49. Muncie, W., *Am. J. Psychother.*, **4**, 595–610 (1950)
50. Murphy, W. F., and Kligerman, S., *Diseases Nervous System*, **11**, 291–97 (1950)
51. Oberndorf, C. P., *Psychoanalyt. Quart.*, **19**, 393–407 (1950)
52. Porter, E. H., Jr., *An Introduction to Therapeutic Counseling* (Houghton Mifflin Co., New York, 223 pp., 1950)
53. Prados, M., *Am. J. Orthopsychiat.*, **21**, 36–46 (1951)
54. Raimy, V. C., *J. Consulting Psychol.*, **12**, 153–63 (1948)
55. Raimy, V. C., Ed., *Training in Clinical Psychology* (Prentice-Hall, Inc., New York, 253 pp., 1950)
56. Randall, G. C., and Rogers, W. C., *Am. J. Psychiat.*, **107**, 422–27 (1950)
57. Raskin, N. J., *J. Consulting Psychol.*, **13**, 206–20 (1949)
58. Rogers, C. R., *Counseling and Psychotherapy* (Houghton Mifflin Co., New York, 450 pp., 1942)
59. Rogers, C. R., Ed., *Client-Centered Therapy* (Houghton Mifflin Co., New York, 560 pp., 1951)
60. Rosen, J., *Psychiat. Quart.*, **21**, 3–37; 111–19 (1947)
61. Rudolf, G. de M., *Brit. J. Med. Psychol.*, **23**, 105–7 (1950)
62. Schofield, W., *Psychol. Monographs*, **64** (311), 1–33 (1950)
63. Sheehan, J. G., *J. Abnormal Social Psychol.*, **46**, 51–63 (1951)
64. Slavson, S. R., *Analytic Group Psychotherapy* (Columbia Univ. Press, New York, 275 pp., 1950)
65. Sterba, R., *Psychoanal. Rev.*, **38**, 75–80 (1951)
66. Stone, A., and Levine, L., *Am. J. Psychiat.*, **107**, 195–202 (1950)
67. Strang, R., *School Rev.*, **53**, 526–29 (1945)
68. Sullivan, H. S., *Conceptions of Modern Psychiatry* (William Alanson White Psychiatric Foundation, Washington, D. C., 146 pp., 1945)
69. Symposium on Areas of Agreement in Psychotherapy, *Am. J. Orthopsychiat.*, **10**, 698–709 (1940)
70. Taylor, F. K., *Brit. J. Med. Psychol.*, **22**, 169–83 (1950)
71. Thompson, C. W., and Bradway, K., *J. Consulting Psychol.*, **14**, 321–23 (1950)
72. Thorne, F., *Principles of Personality Counseling* (J. Clin. Psychol., Brandon, Vt., 491 pp., 1950)
73. Timm, O. K., and Davis, J. E., *Mental Hyg.*, **35**, 69–87 (1951)
74. Torrance, P., *J. Consulting Psychol.*, **14**, 452–57 (1950)
75. Whitaker, C. A., Warkentin, J., and Johnson, N., *Psychiat. Quart.*, **23**, 439–43 (1949)
76. Whitaker, C. A., Warkentin, J., and Johnson, N., *Am. J. Orthopsychiat.*, **20**, 641–48 (1950)
77. Wilcox, P., *Diseases Nervous System*, **12**, 35–39 (1951)
78. Wood, A. B., *J. Consulting Psychol.*, **15**, 72–75 (1951)
79. Young, R. A., Chairman, *Am. J. Orthopsychiat.*, **20**, 311–46 (1950)

XUM

COUNSELING: THERAPY AND DIAGNOSIS[1]

By William M. Gilbert[2]

Student Counseling Bureau and Department of Psychology, University of Illinois, Urbana, Illinois

The broad view of the articles and books which have appeared in this field during the past year is on the whole tremendously encouraging and in parts even exciting. While there are some wide gaps, particularly in the area of relating diagnosis to counseling and therapeutic procedures, there is a general trend towards a healthy integration of varying viewpoints which, while not explicit in some instances, is implicit in the more specific statements made by the various authors.

Possibly the most important trend towards unification is that which concerns the integration of experimental psychology, especially the psychology of learning with psychological therapy generally, and more particularly with psychoanalytic theory and therapy. It also appears that the somewhat artificial barriers between nondirective or client-centered therapy and other more active types of therapy are slowly being dissolved. Likewise, there seems to be a growing recognition that guidance, counseling, and therapy do not represent distinct entities but rather more or less arbitrary points on a continuum relating to the complexity of the problem and the extent of the emotional involvements. With the growing professionalization of counseling and therapy, there is an accompanying instrumentation of the need for assuming professional responsibility, for a guiding set of ethical principles, and for a basic and broad philosophy of psychological counseling and therapy.

While there has not been a great number of research articles regarding the nature of the counseling process, those articles which have been published are basic, and some of them suggest methodological possibilities which may be quite far-reaching. Relatively, there has been a wealth of articles relating to the development, refinement, validation, and critical examination of various diagnostic instruments. Many of these articles show an encouraging concern for careful methodology and for testing some of the fundamental assumptions upon which the instruments are based. This is particularly true in the field of projective devices. However, it is precisely in the area of diagnosis that the greatest gap in knowledge seems to exist. Not one research article has been seen regarding the general problem of relating diagnosis to counseling in a broad way. This is understandable in terms of the

[1] The survey of the literature to which this review pertains was completed about May, 1951.

[2] Grateful appreciation is expressed to R. P. Larsen, Marvin Hersko, and John Noll of the Student Counseling Bureau of the University of Illinois, for their aid in collecting material for this chapter.

great amount of original thinking and labor involved in such research, but it does leave a significant gap which will need to be filled if counseling and therapy are ever to reach a more scientific status.

COUNSELING: THERAPY

General Professional Trends

The problem of the relationship between counseling and therapy is considered either directly or indirectly in all of the books and also in a number of articles relating to counseling. This problem was considered at some length by Bordin (18, 19) and also was touched upon by Pepinsky in his *Annual Review* article of last year (85). Rogers (89), in conformity with his earlier position, uses the terms counseling and therapy synonymously, although he seems to prefer the designation therapy. Thorne (105) accepts counseling as a form of psychotherapy but distinguishes it from "depth psychotherapy," which he feels uses more intensive psychiatric or psychoanalytic methods. Williamson (113) looks upon counseling as being broader than psychotherapy, if psychotherapy is only a curative approach; however, he does accept Shoben's definition of psychotherapy (96) which includes re-education. Wrenn (118), with Dugan (33), Jager (64), and Blaesser (15), re-emphasizes the idea of different levels of counseling, but Wrenn (119) specifically recognizes that, in vocational guidance, there are problems of self-acceptance and that such guidance may be "rationally interpretive or involve dealing with emotional disturbances growing out of a lack of self-acceptance." Robinson (88) accepts therapy as an aspect of counseling with given types of student problems. Guidance and counseling are considered by Blum & Balinsky (17) actually to be therapy. An interesting view of this problem is represented in the books by Dollard & Miller (29) and by Mowrer (80). Possibly it is because of the psychoanalytic flavor of his orientation that Mowrer restricts the term therapy to repressed unconscious processes and more or less summarily waves counseling aside as being synonymous with simple wisdom and advice giving. However, in both books, there is a recognition that repression is not an all or none affair and that there are varying degrees of "consciousness" or "unconsciousness."

Paralleling the problem of the relationship between counseling and therapy is the question of who should conduct psychotherapy. Galdston (49), a psychiatrist, takes an extreme point of view. He says,

> By definition I would have psychotherapy mean the treatment of the abnormal. . . . It follows from this definition that there cannot properly be any such thing as *lay psychotherapy*.

Lindner (70), in reply, takes an opposite point of view when he says

> Psychotherapy is a process of education, as much, and more, than of healing. Its climate is intrinsically educational. Of the therapist it asks that he also be a teacher, and of a patient that he participate in the processes undertaken for his benefit.

Directly related to this problem of the professional status of psychological counseling are the problems of assuming the necessary professional and social responsibilities and of establishing a core of ethical principles for the profession. An extremely significant forward stride towards the solution of this problem is represented by the first report of the American Psychological Association's Committee on Ethical Standards for Psychology (3). The first section of this report concerns the psychologist's responsibility to his client and to society. The Committee feels that the principles developed are relevant to the work of the clinical psychologist, the consulting psychologist, the counselor, and the personnel worker. This article, the first in a series of five, enunciates seven important principles and describes examples of the incidents on the basis of which the principles were formulated. It is obvious that there is a need for additional principles, and it is to be hoped that these will be supplied in the four additional articles. Some of the principles already stated are not as specific or as definite as might be desired. However, the Committee has requested interested persons to communicate with them and to make suggestions. It seems likely that when the time arrives for these principles to be adopted or rejected, they will have been stated in much more precise and positive fashion.

Concern with the upgrading of training for counselors is another significant aspect of increasing professionalization. A number of writers including Rogers (89) and Thorne (105) urge that the counselor in training be given an opportunity for personal counseling. Improvement of practicum and internship training is urged both by the American Psychological Association Committee on Training in Clinical Psychology (5) and by Pierson (86).

Two unique and specific suggestions regarding the teaching of psychological counseling are reported by Thompson & Bradway (104) and by Haigh & Kell (58). The former authors, in view of the increasing awareness of non-content aspects of the interview, such as nuances of tone, the emotional interchange, expressive movements, and so on, describe fake-interview situations in which the students use no words but count or use numbers in any sequence. Haigh & Kell describe multiple therapy as a training method. By multiple therapy they mean the use of two therapists with one client. The authors describe this approach quite optimistically. However, it is evident that there are real relationship problems which arise in such a situation between the two therapists, as well as between the therapists and the client.

Theoretical Contributions

In the major books which have been published, possibly the most meaningful, and certainly the most intellectually exciting, contributions to the whole field of psychological counseling are reflected in the attempt to relate basic personality theory, theories of learning, and theories of perception to the development of maladjustment and to the therapeutic processes that are necessary for the alleviation of such maladjustive processes. The

various authors present points of view which seem to be quite diverse when they are superficially considered. However, on closer scrutiny, the really remarkable amount of agreement with respect to basic issues becomes quite apparent. It would seem that the day may be arriving sooner than anticipated when there will exist among therapists a hard core of general agreement supported by experimentally determined facts and an adequately comprehensive theory of personality and therapy. An example of this tendency towards unification in approach is represented by the fact that Dollard & Miller (29), Thorne (105), Cameron (21), Fisher (45), Wrenn (118), Robinson (88), and Blum & Balinsky (17) all recognize the desirability of providing a friendly, accepting, understanding, empathic atmosphere in which the client may work out his problems. Also, all of these authors recognize that there are circumstances under which it is desirable to become more rationalistic, more interpretive, more instructive. Mowrer (80), whose book does not really concern therapy as such, specifically recommends only an interpretive approach. However, some of his other statements, particularly those on the rearing of children, suggest that he also might agree with the desirability of providing an accepting and understanding attitude. Rogers (89), however, shows no sign of being willing to accept a more interpretive approach. Nevertheless, such an approach would not be basically inconsistent with the general theory of personality which he proposes.

When one begins to consider the approaches of various writers to basic questions concerning counseling and therapy, striking similarities which actually exist, still greater agreements which could easily take place, and also some of the existing differences in viewpoints become apparent. The similarities, differences, and agreements are noted most clearly in: (*a*) the various beliefs as to the manner in which maladjustment arises, (*b*) the general methods of therapy which are suggested for treating such maladjustments, (*c*) the manner in which the transference relationship is conceived and handled, and (*d*) the social implications of the various theoretical approaches.

From the point of view of Dollard & Miller (29), maladjustments always arise when there is a conflict between two strong drives which produce incompatible responses. While symptoms tend to reduce the conflict, they do so only in part, and the symptoms are reinforced because they reduce the "neurotic misery" which results from conflict. When the individual is unable to "label" his own conflicts, he becomes in Dollard & Miller's terms "stupid," and the conflict itself is thereby repressed. Without language and adequate labeling, the higher mental processes cannot then function. However, these authors recognize that sometimes the basic conflicts are merely poorly labeled, rather than not labeled, and thus provide the possibility of degrees of repression and degrees of consciousness.

Mowrer (80), on the other hand, would insist that the kind of "misery" or anxiety which Dollard & Miller discuss is "normal" rather than "abnormal" anxiety, which always involves repression. According to Mowrer,

XUM

maladjustment and neurosis arise as a result of a "learning deficit" which results from the individual's not incorporating social and parental values (that is, conscience or super-ego forces) into the structure of ego. This can occur as a result of a failure in the time integration of punishing and rewarding responses.

Thorne (105) adopts a frankly eclectic point of view and suggests that maladjustment has various causes, among which he includes conflict. He believes that repression occurs in some cases but not in all and that often suppression is mistaken for repression.

Rogers (89) explains conflict from a perceptual frame of reference. In one of the 19 propositions used to explain his theory of personality and behavior he says,

> psychological maladjustment exists when the organism denies to awareness significant sensory and visceral experiences, which consequently are not symbolized and organized into the Gestalt of the self-structure. When this situation exists, there is a basic or potential psychological tension.

Thus, it is apparent that, while using other terms for explaining it, Rogers does accept the presence of conflict and of repression as being basic to psychological maladjustment. It is not clear whether he would make use of a "repressed—not repressed" dichotomy or a continuum. A continuum would seem to be more in line with the general position taken.

Cameron (21) objects strenuously to the "intellectual—emotional" dichotomy and insists that the disadvantages of such a dichotomy far outweigh the gains they may bring. He is not quite so certain about the "conscious—unconscious" dichotomy but finally concludes that it too "imposes many limitations upon our thinking and leads us into using such confusing concepts as 'unconscious anxiety'." In considering these various points of view, it is clear that the way is at least open for formulating a theory of maladjustment which would be equally applicable to the whole of the "normal—abnormal" continuum.

All the authors quoted tend, one way or another, to recognize that conflicts are present, that these may vary in intensity, that repression occurs to some degree, and that "consciousness—unconsciousness" are poles of a continuum. The development of a theory based upon such quantitative differences could do much to clarify the whole field of counseling and therapy, including both training and practice. There are also basic similarities in methods of psychological counseling which can be discerned in the works of the authors just quoted above. Despite the fact that Dollard & Miller (29) recommend a more or less classical free association approach to therapy (just why free association is considered more effective is not quite clear), they emphasize the desirability of providing an accepting, understanding, and nonthreatening atmosphere which will be tension reducing and will tend to eliminate repression and free the higher mental processes for more adequate functioning. At the same time they also believe that the

therapist needs to function as a teacher who, through interpretation, will teach the client more adequate labeling and more adequate discrimination.

Mowrer (80) places a major emphasis upon the role of interpretation, which means that the therapist, after listening to the patient for a while, begins to have associations, to see connections which are forbidden or barred to the patient. By expressing these, the therapist makes them conscious for the patient. Thus, neurotic anxiety is replaced by normal anxiety which will permit the patient to move on towards maturity.

With his eclectic point of view, Thorne (105) recommends a variety of therapeutic procedures ranging from acceptance, reflection, and clarification to interpretation, reassurance, situational analysis and adjustment, suggestion and persuasion, perceptual retraining, semantic re-education, imparting of psychological information, interpretation, and attitudinal reorientation. It is very interesting to note that while Thorne, in terms of space at least and apparently in terms of inclination, gives major emphasis to the more directive forms of therapy, he specifically emphasizes that client-centered or nondirective methods should be used whenever possible, particularly during the early stages of therapy, that all therapy should be client-centered, and that more active methods should be used only with specific indication.

In his new book, Rogers (89) tends to reject his previously held position that it was the therapist's function to help the client recognize and clarify the emotions which he feels. He now holds that this is too intellectualistic. According to his present formulation,

> It is the counselor's function to assume, insofar as he is able, the internal frame of reference of the client, to perceive the world as the client sees it, to perceive the client himself as he is seen by himself, to lay aside all perceptions from the external frame of reference while doing so, and to communicate something of this empathic understanding to the client.

Thus, there is an even greater emphasis on the emotional elements in the therapeutic relationship and an even more radical emphasis on the basic capacity of the individual to make his own choices, uninfluenced by the personal perceptions of the counselor.

When one considers these different approaches to the therapeutic process, it becomes clear that there is a definite trend towards recognizing (*a*) both the emotional and intellectual aspects of therapy and (*b*) both the desirability of providing an understanding, accepting atmosphere and the desirability of teaching the individual how to deal with his problems more effectively. Only Rogers still insists that the counselor should play a relatively uninstructive role and depend wholly upon the capacity of the individual for growth.

In the past, it was customary to consider the problem of transference only in relationship to a deep and prolonged therapy and more particularly in relationship to the psychoanalytic orientation. Today, there is increasing recognition of the role which transference may play in less serious personality

disturbances. It is entirely conceivable that a good deal of the resistance which one sometimes encounters, or of the rejection of test information or other information in vocational counseling situations, is related to this general problem of transference. If one accepts Dollard & Miller's (29) explanation of transference as simply a generalization of responses, the concept becomes much less mysterious and considerably more important in all types of counseling relationships. It is noteworthy that Rogers (89) includes a discussion of transference in his book and that he feels that a mild transference may be present in a majority of client-centered cases, but that deep or dependent transference usually does not develop. In conjunction with normal personality counseling, Thorne (105) too recognizes the role which transference may play and briefly suggests procedures for handling it.

When one considers not only the degree to which a person may be adjusted within himself, but also the degree to which he is adjusted to society, the sharpest differences in the various approaches occur. Dollard & Miller (29) recognize the importance of the individual's adjustment to society when they relate ego strength to the utilization of higher mental processes and to the acquisition of culturally valuable, learned drives and skills. However, as one might expect, they also lean heavily towards drive reduction, that is, the satisfying of an individual's needs whether these be culturally learned needs or more basic, primitive ones. Mowrer's position (80) is extreme in the opposite direction as indicated by his belief that neurosis itself is basically a learning deficit and that this learning deficit consists in the nonincorporation of super-ego forces, of conscience, and ultimately of societal codes and mores. It is almost as if Mowrer would expect the therapist to be a rather stern, uncompromising, and disciplining father figure. Thorne's position (105) is almost equally extreme. He says, "Established legal decisions have consistently given greater weight to the rights of society as compared to the rights of an individual." Many of Thorne's more directive procedures are concerned with helping or even forcing the individual to adapt to the demands of society.

Cameron's point of view (21) is considerably more inclusive. He believes that, in terms of our growing knowledge of behavior and of other cultures, it is impossible to set up absolute values or norms and that instead we should accept a more relativistic conception of human behavior. Rogers' phenomenological approach (89) places exclusive, or almost exclusive, emphasis on the individual in his adjustment within himself. Rogers says,

> Thus, while the establishment of values by each individual may seem to suggest a complete anarchy of values, experience indicates that quite the opposite is true. Since all individuals have basically the same needs, including the need for acceptance by others, it appears that when each individual formulates his own values, in terms of his own direct experience, it is not anarchy which results, but a high degree of commonality and a genuinely socialized system of values.

One may question this final conclusion if one assumes that many of the important needs which are concerned with adjustive processes are learned

needs and that these may vary considerably according to the culture or the subculture in which the individual lives. The difficulty of an individual developing broader cultural values is heightened, it would seem, if one insists as Rogers does that an individual should not introject the values of other persons, but only those values which are experienced directly. This would seem to mean that an individual would need to experience for himself all of the accumulated events which have given rise to our present laws and social mores. Pepinsky (85) criticizes the "internal frame of reference" approach as solipsistic and as a "rededication to the subjective idealism of prescientific psychology." Luchins (72) in his criticism of Bruner & Postman's phenomenological approach to social perception (19a) concludes by saying,

> It seems to us that Bruner and Postman tend to ignore the roles played by reason and intelligence in the perceptual process. Their perceiver strikes us as being a hedonistic, striving, conative being—*homo volens* as opposed to *homo sapiens*.

Williamson (113) is similarly positive in his objection to this approach. He says,

> As with Rousseau, some non-directivists seem to take the position that human nature would appear to be essentially "good" and society is essentially "wicked" in its "imposition" upon the natural growth of the individual. Such adoptance stems from a concept of relationship between interdynamic forces of the individual and the outer societal forces of his social environment which, like Rousseau's doctrine, maximizes the internal forces and minimizes the contributions of the external forces.

Thus, while there are differences in emphasis among the different authors, there also seems to be general agreement that therapy needs to take into account both the individual's adjustment within himself and his adjustment to society in one way or another.

From all of the foregoing discussion, it appears that counseling is moving strongly towards a really professional status, that any distinctions between counseling and psychotherapy seem to be basically unnecessary and artificial, that the therapeutic situation represents a teaching-learning relationship, and that there are broad areas of theoretical agreement which may provide the basis for the future development of a broad theory of personality development and of therapy which will be applicable to the whole continuum of psychological adjustment.

Analysis of the Counseling Process and Relationship

A number of recent studies relate to some of the fundamental assumptions with respect to counseling, to certain aspects of the counseling process, and to the nature of the counseling relationship. With respect to the last category mentioned, a new research methodology has been utilized which should open the way for a great number of investigations previously considered to be impracticable. One of the fundamental assumptions made with respect to practically any counseling procedure is that somehow or other the verbal exchanges occurring in the interview situation will actually

XUM

influence adjustment and behavior generally. Three studies relate directly to this question.

Barry (9) has studied the relationship between verbal reactions in interview protocols and global judgment of adjustment level based upon the same protocols. The author made a content analysis of the protocols of the initial and terminal interviews, and the results of this analysis were compared with adjustment ratings. Among other aspects of content, indication of perceived strength of, or threat to, self was utilized. This was found to be significantly associated with the ratings of adjustment level. One may question whether the ratings of adjustment level, since they were based on the interviews, were really external criteria. Barry recognizes this but argues that the criterion is a relatively external one because it was based upon reported behavior rather than reported affect and because the judges were not participants in the counseling and interviewing procedures.

A great many writers, particularly those with client-centered orientation, assume that counseling brings about changes in self-conceptualization which are then reflected in behavior. Benjamins (12) reports a very neat and interesting experiment in which he had a group of high school students first rank themselves on intelligence and four other characteristics and then take a group intelligence test. The following day they were given false reports of their ranks on the test, after which they reranked themselves and took an alternate form of the test. After the second test a questionnaire regarding their reactions to the false reports was administered. On the basis of the change in self-rankings and the pattern of questionnaire responses, the direction of change in test score was predicted for each subject with the use of reaction categories. Differences were significant beyond the 1 per cent level. Questions may be raised whether the methods of influencing self-conceptualization recorded here are parallel with those influences which occur in a counseling situation and whether behavior, which would follow later than was the case in this experiment, would be similarly influenced.

Pace (83), in a study of opinion and action, compares recorded opinions about philosophy in religion, in art and music, and in politics with recorded action in these same areas. He obtained correlations in the neighborhood of .20 and .30 between belief in the importance of a field and reported participation in activities in that field. Again, while this study is only suggestive of what might occur in a counseling situation, it does warn against the uncritical acceptance of client's statements about themselves as valid criteria of changes in adjustment.

In view of Rogers' present emphasis on the importance of empathy in a counseling relationship, Dymond (34) attempts to determine whether differences in empathic ability are related to certain personality variables. Her positive results can be considered only as suggestive of such differences because of the small groups employed and because no real statistical comparison of the two groups could be made.

Important studies relating to the analysis of the interview have been

made by Dressel & Matteson (31) and by Elton (35). Dressel & Matteson studied the effect of client's participation in test interpretation. They conclude that students who participate most, gain most in self-understanding, tend to be more secure in their vocational choice, and are neither more nor less satisfied with counseling.

Elton's study concerns the relationship between the amount of responsibility-taking by a client in a counseling interview and certain interview outcomes. A breakdown of responsibility ratings suggest that middle ratings of responsibility tend to be accompanied by greatest gains in insights and that a harmonious relationship between the client and counselor is most likely to be obtained if responsibility for the direction of the interview is shared by counselor and client. These latter conclusions are not as clean-cut as one might wish, but they are suggestive. Elton also shows that the degree of responsibility assumed by the counselee is affected by the counselor's technique and that, when the effect of counselor technique is controlled by means of derived scores, there is still a significant positive relationship between responsibility-taking and the variables of insight and working relationship.

One of the extremely important problems in psychological counseling and therapy is that concerning the need for being able to describe and quantify different types of psychotherapeutic relationships. The possibility of accomplishing this is basic to further studies concerning the relative effectiveness of different types of counseling procedures. Studies of this nature will be necessary before it will ever be possible to make any scientific statements regarding such basic questions as the relative effectiveness of different counseling procedures with different types of counselee problems or the relative effectiveness of a single over-all approach to all types of counseling problems as compared with diverse approaches based upon various diagnostic categories.

In a series of four articles, Fiedler (41 to 44) presents not only some extremely important and interesting findings, but also the application of a methodology for quantifying certain aspects of psychotherapeutic relationships. In his first study, Fiedler points out that there have been wide differences of opinion between representatives of rival schools concerning what constitutes a good therapeutic relationship. He questions whether these are merely differences in semantics or whether they represent actual divergencies in the goals therapists set for themselves. Fiedler argues that, if the differences are real, then factor analysis should yield as many factors as there are points of view, but if they exist only in theory, then only one type of relationship actually will be considered to be maximally effective; and moreover he argues that divergencies among therapists of different schools would be expected to exceed those between therapists representing a single school, provided that training is important. If experience and skill are of greater importance, and only one type of relationship is discovered to be maximally effective, it should be expected that experts would agree

XUM

among themselves, irrespective of school, to a greater degree than with nonexperts representing their own schools.

In a preliminary study, Fiedler used eight therapists, four of whom were psychoanalytically oriented, two of whom were nondirectively oriented, and two of whom considered themselves to be eclectic. Also there were differences in the amount of training of these three groups. Next a series of 119 qualitative statements descriptive of patient-therapist relationships were selected from the literature and given to the eight therapists to be sorted into seven categories ranging from those statements regarded as "most" to "least" characteristic of their own conceptions of an ideal therapeutic relationship. Each statement received a score of from one to seven, depending upon the category in which it was placed. This is in accordance with the Q-techniques described by Stevenson (100). The ratings by each therapist were then intercorrelated with those of each other therapist, and the resulting matrix factor analyzed. The analysis yielded only one general factor. An inspection of individual items also revealed no major differences between schools. In another experiment, which was more carefully designed, seven different subjects co-operated in sorting a more carefully selected array of 75 statements about therapeutic relationships. These statements were sorted into seven categories, as before, by three psychoanalytically oriented therapists, three nondirectively oriented ones, one Alderian expert, and three untrained persons. The results confirmed those of the first study, showing that there was only one general factor. Fiedler concludes from these two studies that: (*a*) therapists from different schools do not differ in describing the concept of an ideal therapeutic relationship; (*b*) the ability to describe this concept is probably a function of expertness rather than theoretical allegiance to any one school; (*c*) nontherapists can describe the ideal therapeutic relationship in the same manner and about as well as therapists; the therapeutic relationship may, therefore, be but a variation of good interpersonal relationships in general; and (*d*) a composite rating of the ideal therapeutic relationship can be obtained by means of pooled ratings. Fiedler quite rightly emphasizes,

> The findings suggest that therapists generally agree on the most effective type of therapeutic relationship and that theoretical differences are the result of poor communication between therapists of different schools.

In a second study, logically related to the first, Fiedler (42) studies the degree to which different therapists actually achieve the goal of an ideal therapeutic relationship and, if they differ in the relationship which they create, what particular aspects of the relationship are different. In this study, three trained judges and one untrained judge listened to 10 recordings, each being that of a single interview selected from somewhere between the sixth to seventeenth hour of treatment of 10 different patients. Each of these interviews had been conducted by one of 10 psychotherapists representing the psychoanalytic, the nondirective, and the Adlerian schools of

therapy, and so chosen so that at least one from each school could be considered an expert and one could be considered a novice. Fiedler's findings were as follows: (*a*) experts created a relationship closer to the ideal than that created by the nonexperts; (*b*) the relationships created by experts of one school resembled more closely that created by experts of other schools than it did the relationship created by nonexperts within the same school. Of the three dimensions of therapy measured, which consisted of (*a*) communicating with and understanding patients, (*b*) maintaining an emotionally proper distance, and (*c*) divesting themselves of status concern in regard to their patients, it was found that the most important dimension which differentiated experts from nonexperts related to the ability to communicate with patients. To a lesser degree, they were differentiated by ability to maintain an appropriate emotional distance. Differences between schools were most apparent in terms of the status which the therapists assumed towards the patient, the Adlerians and some of the psychoanalytically oriented therapists tending to place themselves in a more tutorial role, the nondirective therapists tending toward the opposite direction. Fiedler recognizes a number of limitations to his study which include the fact that subjects were not assigned to the therapists at random, that only one interview per therapist was used, and that there may be still other dimensions of therapy in addition to the three that were utilized.

In his third study, Fiedler (43) factor-analyzes the sortings obtained in the study just described. Several factors were found which clearly differentiated experts from novices. However, only one factor was found which tended to differentiate between members of different schools. This concerned the status of the client in relationship to the therapist. Thus there is additional evidence for Fiedler's major hypothesis that the therapeutic relationship is a function of expertness rather than of a particular theory or methodology. In his final study, Fiedler (44) presents a method for quantification of certain counter-transference attitudes by using the Q-sort procedure.

These four studies of Fiedler have provided a new methodological approach. Since differences between therapists of different schools, at least in terms of the dimensions which are utilized, do not seem to exist, a theory of personality structure and of therapy more general than any propounded by adherents to the schools which Fiedler has tested would seem to be urgently needed.

Group Counseling and Therapy

Contrary to Pepinsky's rather optimistic view of last year (84), very few studies related to group counseling have appeared. Hobbs (61) in his chapter in Rogers' book describes a very interesting application of the client-centered or, more properly speaking, the group-centered approach to group therapy. He illustrates the nature of group-centered therapy and points out the similarities and differences between individual and group therapy. Hobbs also reports on a study by Gorlow (54) which relates to the activities

of group members as therapists. He finds that interaction between group members changes in quality as therapy progresses, that members seem to learn to become better therapists, and that there is an increase in permissiveness and acceptance, and a decrease in interpretive, evaluative, and critical remarks from the first to the last part of therapy. Gorlow also finds that individuals who are best adjusted before entering therapy are those best able to adopt the constructive attitudes of the therapist in the initial sessions.

That group therapy may find rather wide application, once it becomes more widely accepted and more psychological therapists become interested in carrying on group work, is suggested by a report of Coffee, Freedman, Leary & Orsorio (25) in which clinical psychologists carried out group psychotherapy in conjunction with church problems of collaboration and planning. Gordon (53) describes the utilization of group procedures in creating an effective faculty advisor training program, and Kaplan (66) reports on a group psychotherapeutic approach with expectant mothers which was used by Tasker Center, in Jerusalem.

Evaluation of Counseling Outcomes

While the problem of evaluating counseling outcomes has received considerable attention during the year, there have been no really well controlled studies utilizing valid external criteria and unselected control groups. A number of general problems are discussed in papers presented at the 1950 Convention of the American College Personnel Association.

Strang (101) discusses some of the major limitations in current evaluation studies. She expresses doubt that insight as expressed in the interview is adequate evidence of adjustment, and she emphasizes that real evidence should come from performance in life situations. Gilbert (52) points to some of the administrative problems in human relations which are involved in any large scale evaluation program and suggests that the co-operation of all persons concerned should be obtained before attempting to carry out evaluation on a large scale. Friedenberg (48) describes a clever and detailed attempt to evaluate the attitudes of students towards a college advisory system. His approach utilizes the statement of problem situations as a major aspect of his procedure. A brief inventory of student reactions to all types of student personnel services was described by Kamm (65). His questionnaire is so brief that there is considerable question as to the amount of usable information which would be obtained from using it.

Only two studies relating to the evaluation of counseling make use of any sort of control groups or otherwise take into account the methodological considerations pointed out by Williamson & Bordin (114). The most complete and comprehensive study presented is that by Rothney & Roens (90). They made a longitudinal study of guidance and counseling outcomes among 129 counseled pupils, as compared with outcomes of 129 paired, uncounseled pupils over a span of five junior and senior high school grades.

The principals in junior high schools selected for the experimental group, those students who "needed help" and those who were "superior." The uncounseled group was selected later, and the selection was contaminated by use of teachers who were being encouraged to promote the guidance program in this school system; thus, the control groups were not as well selected as one might wish. The authors' findings suggest that the counseled group made significant gains in terms of academic outcomes, such as less subject failure, fewer curricular changes, higher grades, and higher rate of admission to colleges. Gains were also found in terms of "student—reported" outcomes, such as greater employment among noncollege, guided groups, greater definiteness concerning vocational plans and means of attaining them, and better adjustment on a job.

Barahal, Brammer & Shostrom (6) counseled with matched groups of volunteer (nonveteran) college freshmen. One group was counseled in terms of what the authors call a client-centered approach, the other in terms of the usual veterans' administration procedures. The authors found that the client-centered counseling procedures resulted in more positive client satisfaction with counseling and in more self-direction. This study does suggest the possibility of utilizing information techniques such as tests, occupational information, and personal histories in conjunction with a permissive client-centered approach to counseling.

Reporting on a six months follow-up of the effects of personal adjustment counseling of veterans, Bartlett (10) had training officials, who were responsible for supervising the veteran trainees before, directly after, and six months or more subsequent to counseling, evaluate the veterans' progress. The unselected sample of 498 cases was rated six months or more after counseling; 30 per cent were rated as much improved, 44 as showing some improvement, and 18 as showing no improvement. It was also found that the average number of interviews was related to improvement. The authors infer that nondirective techniques are well suited to brief psychotherapy for cases with minor maladjustment. In making any proper evaluation of this study, one would wish to know to what extent the training officers were aware of the fact that the trainees had received personal counseling, to what extent their opinions were influenced by this knowledge, and to what extent they might be influenced in a relatively unconscious way at least to make favorable ratings. In another experiment wherein judges rated the success of counseling, Blau (16) attempted to predict the success of counseling in seven cases on the basis of a classification of client's statements taken from first interview recordings. While regarding his results as promising, Blau recognizes that the sample is too small and that real conclusions must await the results of a larger, better controlled study.

Barnette (8), and also Gandet, Carli & Dennegar (50) report favorable results from questionnaire studies of veterans who had received counseling. While such studies may be useful administratively in determining the attitude of clients toward a counseling service, there is considerable question as

XUM

to how much scientific information they supply regarding the success of counseling.

COUNSELING: DIAGNOSIS

It would have been extremely satisfying to have been able to write this whole chapter on counseling in such a way that the published materials relating to diagnosis and counseling could have been interwoven and integrated to present a consistent and coherent picture of the total process. This will undoubtedly occur at some time in the future. At the present time, however, it seems much more likely that the sections dealing with psychotherapy and with counseling will be combined far in advance of the time when therapy and diagnosis will be integrated. Berdie's question (13) in the *Annual Review of Psychology* for 1950. "What diagnostic categories and techniques are most useful in selecting appropriate therapies?" is still an unanswered cry in the wilderness. Pepinsky's earlier monograph (84) seems to be about the only experimental work on this problem as it relates to the more normal type of adjustment difficulties. The monograph is well deserving of the recognition it has received.

General Problems of Diagnosis

In his article concerning diagnosis, published last year in the *Annual Review of Psychology*, Stuit (103) said,

> It is, in other words, not a question as to whether diagnosis shall be used in counseling. It is rather a question of what techniques should be used, when they shall be applied, and how they shall be interpreted.

Practically all of the books except Rogers' quite specifically emphasize the importance of both a general diagnosis and the imparting of diagnostic facts. This is true of the books by Robinson (88), by Wrenn (118), by Blum & Balinsky (17), and by Thorne (105), as well as the more psychiatrically oriented books by Fisher (45) and Cameron (21). While Stuit's statement above does not seem to be perfectly valid, it does seem to represent the majority opinion. Robinson discusses the categories investigated by Pepinsky and then suggests three of his own, which include: adjustment problems, skill problems, and maturity problems. Robinson, however, presents no evidence based on research as to the real efficacy of his categories, and until some such evidence is forthcoming they should be considered simply as tentative suggestions. From an empirical point of view, there would seem to be some reason to question the distinctness of the categories suggested as well as their paucity.

Thorne has probably devoted more attention to the general problem of diagnosis in counseling than anyone during the past year. In discussing diagnosis generally, he says, "Making a diagnosis is the largest part of the problem, since after the causes are identified, one knows what to do." He does, however, also recognize that diagnosis is a continuing process which accompanies therapy. Thorne also suggests different procedures for the dif-

ferent diagnostic categories which he suggests, and he enlarges on the standard classifications of mental disorders to include additional categories which have not been traditionally dealt with. His seven major classifications of behavior disorders are as follows: (*a*) reactions of organic defect, (*b*) reactions of constitutional personality inadequacy, (*c*) reactions of physiological deficit, (*d*) reactions of psychological deficit, (*e*) condition behavior disorders, (*f*) attitudinal pathoses, and (*g*) situational maladjustments. The first three of these major groupings relate to the usual organic, psychoneurotic, and psychotic disorders; the last four, to less serious disturbances. Fisher, in his book, discusses quite dogmatically the types of therapeutic procedures which he feels are applicable to persons suffering from the disorders which are classified under the usual psychiatric categories. Cameron devotes very little discussion to diagnosis as such, although he does suggest criteria for distinguishing between persons who can make best use of what he calls "integrative psychotherapy" and those with whom more directive types of psychotherapy may profitably be used.

It is clear, however, that there is still great need in the area of diagnosis for the experimental establishment of additional useful diagnostic categories of a causal nature particularly in the area of the more "normal" problems which are encountered in most psychological counseling services. The importance of diagnosis in counseling is epitomized by Miller's statement (79):

> The basic design for evaluating a total therapeutic course or any segment of it must be: diagnosis of the condition before the therapy; measurement or description of the procedures employed; diagnosis of the condition afterward, in order to measure the change.

Diagnostic Devices

A great deal of work has been done during the year relating to the refinement, validation, and improvement of the various diagnostic measures. Complementing the increased interest in personality and emotional factors in counseling, we find that well over half of all of the articles relating to testing are concerned with measures of personality and more particularly with the various projective devices and the Minnesota Multiphasic Personality Inventory (MMPI). The remainder are concerned with interest tests, with measures of general intellectual aptitude and achievement, and with selection procedures.

Sarason (92), in his discussion of the test situation and the problem of prediction, has stated the general problem clearly:

> Until there are attempts on the level of theory to identify the determinants of behavior, to state the principles by means of which they become related and how they can become measured, putting one's eggs in the basket of "conventional clinical procedures" is not likely to pay off.

Darley & Anderson (27) also emphasize the importance of measurement, when they say:

Psychological measurement functions in this counseling process as a means to an end: to help identify individual strengths and weaknesses within an individual and between individuals relative to competitive levels; to provide insight and understanding for the student and the counselor; to structure diagnostic descriptions either in cause effect terms or in terms of covariation of behavioral elements; and to permit more accurate predictions than would otherwise be possible.

Possibly the one publication which will be of most significance to the whole field of diagnostic testing is the report concerning ethical standards for the distribution of psychological tests and diagnostic aids by the American Psychological Association's Committee on Ethical Standards for Psychology (4). The Committee lists 18 principles under the broad categories of: (*a*) qualifications of test users, (*b*) role of a psychologist sponsoring test activities, (*c*) role and qualifications of test publisher's representative, (*d*) choice of distributor for a new test, (*e*) readiness of a test for release, and (*f*) description of tests and manuals and publications. Altogether 18 well stated, specific and positive principles are suggested. It seems probable that most of these principles, if not all, will be accepted by the various state psychological associations and that, once adopted generally, they should stimulate greatly the more adequate use and development of various diagnostic devices.

Of broad general interest and of special concern to psychological counselors and psychologists generally are the first two reports concerning the prediction of success in the Veterans Administration training program in clinical psychology by Kelly & Fiske (67) and by Tupes (108). The carefully thought out research program, detailed experimental design, thorough analysis of results, and open-mindedness with which these are discussed may well serve as models for future selection and prediction studies.

Kelly & Fiske, taking the results from a variety of objective, projective, and situation tests and the ratings obtained from a number of interviewers who possessed varying amounts of test data and other information, correlate these with data on training status and evaluations by university clincial staff members and Veterans Administration supervisors. Some of the more interesting results were as follows: (*a*) The Miller Analogies Test yielded statistically significant differences between those who completed their training and those who were dismissed. (*b*) The prediction of evaluations by supervisors yielded data showing that the validity of all predictive measures tends to be low, the highest being .48, and the majority being statistically not significant. (*c*) The most efficient prodictions both in terms of validity and economy of data are those based only on the materials contained in the credentials file and in the objective test profiles. The addition of neither autobiographical nor projective test data contributes significantly to the validity of the assessment ratings, nor do initial or intensive interviews. "In fact the predictions based on the credentials and objective tests are better than those made at the end of the program on the basis of all tests, procedures and observations!" (*d*) The Strong Vocational Interest Blank Veterans Administration Clinical Psychologist Key regularly yielded high correlations

with all criterion evaluations and compared favorably with the best predictions based on assessment ratings. The authors conclude, "Thus, scores from a single objective test obtainable by mail, at little cost, predicted each of several criteria as well as any of the clinical judgements made in the entire assessment program."

In Tupes' study, an evaluation was made of personality trait ratings obtained in the unstructured assessment interviews. The study included 128 Veterans Administration trainees. The final pooled personality ratings and skill ratings were correlated with (*a*) the preinitial interview ratings, (*b*) the postinitial interview ratings, (*c*) the preintensive interview ratings, and (*d*) the postintensive interview ratings. Tupes' conclusions are as follows:

> 1. The contribution of the unstructured appraisal interview . . . plus psychological data is slight. 2. Longer interviews . . . contribute little, if any, more to the validity of personality-trait ratings than do shorter interviews . . . . 3. The more comprehensive the psychological data available, the more valid will be personality-trait ratings based on that data. 4. Personality-trait ratings based on fairly comprehensive psychological test data are more valid than are ratings based on interviews with only a little psychological data in the form of credentials material available. 5. At the present time, even skilled clinicians, having available for study and integration a wide variety of objective test data, projective protocols, credential material, and autobiographical data, plus data accruing from a probing face-to-face interview, do not appear able to make personality assessments, in the form of ratings of personality traits, which have sufficient validity to be as useful in individual case work as is desirable.

While the results of these two studies are indeed disappointing in terms of the noneffectiveness of clinical interview ratings, of the situation tests employed, and of the projective tests, they are encouraging in terms of the contribution of relatively easily obtained objective test results. These carefully carried out studies should serve as serious cautions to anyone making predictions on the basis of clinical impressions or on the basis of projective devices. The large amount of work now being carried on in testing the fundamental assumptions and the validities of the various projective tests should be extremely useful in determining which aspects of these tests, if any, are worth-while and which are not.

Luft (73) presents a study which is basically related to that of Tupes, described above. Luft had two groups, one composed of clinical workers (9 psychiatrists, 12 social workers, and 9 clinical psychologists) and the other composed of five physical scientists, listen to a case conference summary by a patient's therapist, and then to predict how the patient would respond to a standardized personality questionnaire. He found that, while one-third of all judges exceeded chance scores at the 1 per cent level of confidence, there were no significant differences between the two groups. In the second part of his experiment, Luft had the judges listen to a 1-hr. diagnostic interview with each of two different patients and then predict the patients' responses to an objective test as well as to identify the patients' responses to sentence

XUM

completion items. The over-all results agreed with those of the first experiment with the prediction scores on the sentence conpletion test being relatively lower than on the objective test.

Projective Procedures

Rorschach.—There are a great many conflicting opinions with respect to the use of the Rorschach as a diagnostic instrument. These range from damning and belittling statements, made privately by individuals who feel that the Rorschach is too subjective for any real use, to opinions which exalt and praise the Rorschach as one of the few really useful clinical instruments. Some of these strong feelings even creep into published articles. Schofield (94), in reviewing the research in clinical psychology for 1949 says,

> Rorschachers who hold to the "party line" will reject researches such as these as atomistic and negligent of the interdependence of scoring variables and the configurational interpretation of records. It seems improbable that any kind of configurational metering of interrelated variables will result in usefully valid and *public* interpretative concepts if the underlying variables lack any demonstrable validity. At any rate, it appears that the honeymoon of the Rorschach is over. It is not likely to be divorced by the ardent clinician, but it is now clearly out of the darkened ballroom and under the bright lights of the psychological kitchen where its products will be evaluated by increasingly critical palates.

Korner (69), on the other hand, in discussing the scope and limitations of projective techniques, implies that, if Rorschach results do not correspond to clinical findings, then the clinical findings should be suspect since it is probable that the Rorschach results point to basic personality tendencies which have not yet become overt.

A somewhat more reasoned approach to the problem of Rorschach validation is supplied by Schneider (93) who reviews the different approaches and says,

> Rorschach validating procedures can be most fruitfully treated as problems in relating Rorschach variables to independent measures of component personality processes [and] this does not do violence to a pattern interpretation, but there should be a specific statement of any particular constellation that is believed to be related to some aspect of behavior.

A number of studies which conform with Schneider's recommendations have been made. Holtzman (62) studied two groups of college men who could be considered normal superior adults. He used pooled judgments as criteria for determining shyness and gregariousness and then compared the shy group and the gregarious group in terms of Rorschach results. He used both intuitive blind analyses made by two Rorschach experts as well as very complete qualitative and quantitative analyses, of objective Rorschach signs. All attempts to find relationships between the Rorschach and the two distinct groups failed. Tucker (107) reports on the relationship between Rorschach, human, and other responses and intelligence as measured by the

Wechsler-Bellevue. He found that M and FM plus m responses correlate at the same level with intelligence but that neither correlate high enough to be used as predictors of intelligence.

In another article Holtzman (63) found that impulsiveness as judged by intimate associates of college men was not related to isolated ratios in the Rorschach. However, when he weighted 23 different Rorschach "signs" of impulsiveness as empirically derived from a part of his group, he obtained a correlation of .60 between the test and criterion data when he used the same group from which the signs and weights were obtained. On a cross validation study, a correlation of .49 between the test and criterion data was obtained.

Dubrovner, Lackum & Jost (32) studied the effect of color on productivity and reaction time in the Rorschach. They presented achromatic reproductions of regular Rorschach cards and also regular Rorschach cards to two groups of subjects, reversing the order of presentation of the two sets of cards with the two groups. They conclude that: (*a*) there is no evidence to support the hypothesis that color affects productivity; (*b*) there is no support to the idea that the percentage of responses to color cards VIII, IX, and X can be used as an index of emotional resources; (*c*) there is no support for the assumption that color effects reaction time; and (*d*) since fluctuations in difficulty of cards occur with the achromatic presentation, such fluctuations cannot be attributed solely to the effects of color—this result adds a complicating factor to the concept of color shock. A factor analysis of 21 of the basic scores of the Klopfer system was done by Wittenborn (116). The obtained intercorrelations are accounted for by four factors; after rotation, several clustering tendencies became conspicuous. Some of the findings agree and some disagree with practices in clinical interpretation.

In conformity with the current trend of emphasizing content of Rorschach responses, Meer & Singer (76) investigate the assumption that Card IV is the "Father" card and Card VII is the "Mother" card. The results show that Cards II and IV were chosen as "Father" cards and Cards VII and X as "Mother" cards with frequencies which were significant at the 5 per cent level or above. The effectiveness of the Rorschach in distinguishing homosexuality in male college students is studied by Fein (40). Using a sample consisting of 9 homosexuals, 10 anxiety neurotics, and 24 normals, he investigated 15 suspected Rorschach signs. From the data he concludes that (the following) responses are homosexual indicators: feminine apparel and behavior, men with feminine attributes, M with sex connotation, male and female in symmetrical blot areas, and M with sex aversion. Other responses such as castration and phallic symbolism become indicators of homosexuality only when these responses pile up in a record, while feminine gender responses are not indicators of homosexuality. Paranoid reactions and confusion of sex identification are more indicative of insecurity than of homosexuality in male college students. Another Rorschach assumption is tested by Carp (23) who compared teachers' ratings of constriction with Rorschach

protocols, crayon drawing, and play construction. Her results show no consistency from test to test or with the behavioral criteria.

Sacks & Lewin (91) discuss the limitations of the Rorschach as a sole diagnostic instrument. They object to the use of "signs," claiming that it is too mechanical, that blind diagnosis and personality description are at best "stunts" in clinical practice, and that as supplements to Rorschach findings, the interview, the case history, the results of the Thematic Apperception Test (TAT) or other instruments should also be used. In view of the Kelly & Fiske findings (67) in the Veterans Administration program there seems some reason to doubt whether the addition of more projective tests would increase the accuracy of diagnosis.

The general trend of the findings with respect to the Rorschach suggest the need for caution in interpreting Rorschach scores, in making diagnoses on the basis of them, or at the present time of trying to fit therapy to personality as it is described by the Rorschach procedure. It is to be hoped that, with the increasing amount of evidence regarding the Rorschach, there will also be more caution on the part of both clinical psychologists and psychiatrists in relying too heavily on Rorschach or other similar tests in determining whether or not a person should be hospitalized and particularly whether some of the more drastic treatments, such as shock therapy, psychosurgery, and the like, should be used.

Thematic Apperception Test.—A number of studies have been made of the TAT, the results of which also suggest caution. Eron, Terry & Callahan (36) had 2,000 TAT stories, contributed by 50 male and 50 female college students, rated for emotional tone on an empirically derived scale. The stories tended to be sad, and there was considerable difference between cards as to the mean sadness or gladness elicited, indicating that the TAT cards have stimuli values of their own which help determine the emotional tone of the stories given in response to them. Wittenborn & Eron (117) used data from 50 male college students and 50 male neurotic patients in computing correlations between range of emotional tone and outcome ratings for the same card and for different cards. They found that the emotional tone of responses to the TAT seems to be determined largely by the card itself. This finding confirms the previous study. Nevertheless, the outcomes of the stories seem to be relatively independent of the card and consequently may be used to provide information about the affective level of the subject.

Calvin & Ward (20) attempt an experimental validation of the TAT by selecting groups of "high certainty" and "low certainty" persons on the basis of their certainty of reaction to a light-buzzer combination which actually occurred by chance. From the total group of 90 subjects, the 10 highest and 10 lowest in certainty were chosen. The TAT results were then analyzed by the Murray need system procedure and for language usage and desirability of outcomes. Few striking, but several suggestive differences between the two groups were found. Eron & Ritter (37) describe a group administration of the TAT and compare it with the individual administration. Bellak,

Levinger & Lipsky (11) describe differences in two administrations to an adolescent girl, who without therapy during the interval between the two testings solved some of her personal problems. The improvement, which was observable on the TAT, suggests that this test might be used to reflect maturational processes in adolescents. A telling point is made by Reiss (87) when he compares results of the Negro and white TAT cards given to Negro and white subjects and administered by Negro and white persons. He finds no statistically significant difference in the productivity of Negroes with the Negro stimulus cards as compared with the white stimulus cards and concludes,

> the substitution of negro skin color for white skin color, however, in the ambiguous social situation of the TAT card does not facilitate the response of negro subjects; it may in fact impede it.

Szondi.—Most of the research reported on the Szondi test yields findings which indicate that the Szondi should be used clinically with extreme caution, if at all. Cole & Roberts (26) show that different pictures within the same clinical group produce decidedly different group reactions, while Guertin (56) (57) finds that Szondi pictures and a set of pictures of nonmentally ill Americans were ranked in such a way as to demonstrate no superiority of the Szondi pictures and that there is high picture-for-picture agreement which cannot be explained by Szondi's need systems. Guertin urges that factor analysis be used to establish new need-system variables. Fosberg (46) presents the results of four experiments relating to certain problems of validity and is able to point to three findings which tend to support Szondi's theory. However, he also points out five significant findings which do not support it and concludes, therefore, that until experimental evidence is brought forth to answer all the pertinent questions which have been raised, the Szondi test in its present form should not be used in a clinical situation.

New projective devices.—Ammons, Butler & Herzig (1) suggest an unusual and promising use of projective procedures for use in vocational research and guidance at the college level. The authors have prepared a set of 18 plates portraying individuals engaged in activities characteristic of different occupations. They have developed methods for identifying and scoring general attitudes towards an occupation. The consistency of scoring was high, and a low but statistically significant positive relationship was found between the Strong Vocational Interest Test and this Vocational Apperception Test.

Harrower (59) asked subjects. "What is the most unpleasant thing you can think of?" and then said, "And now I want you to draw it for me." This procedure, she feels, is particularly useful for high-lighting specific problem areas and for discovering underlying schizophrenic processes. A somewhat similar approach was used by Meltzer (77). He simply asks clients (*a*) to recall the most outstanding pleasant and unpleasant experiences they have had, (*b*) to state who was the greatest person that ever lived and why, (*c*) to say who now is the greatest person living and why, and (*d*)

XUM

to state whom, of all the people they have ever seen, heard or read about, they would prefer to be like and why.

The most radical suggestion yet offered by anyone seems to be that made by Dörken (30), who proposes that instead of using a standardized stimulus like the Rorschach cards, the clients should manufacture their own ink blots and react to them. This seems like carrying projective procedures to a point where they might well deserve a great deal of the censure which has been heaped on them by persons who object to their nonobjectivity.

Minnesota Multiphasic Personality Inventory and Related Tests

The imposing and comprehensive *Atlas for the Clinical Use of the MMPI* by Hathaway & Meehl (60) presents a collection of 968 short case histories with one or more associated MMPI profiles for each. This volume should be of considerable aid in interpreting the shape of MMPI profiles obtained from clients and comparing them with those represented in the *Atlas*.

A number of articles provide worthwhile information for the use of the MMPI, Gough (55) demonstrates the usefulness of the F minus K dissimulation index, Winne (115) describes a scale of neuroticism adapted from the MMPI, while Wiener (112) provides evidence that subtle (S) and obvious (0) keys for the MMPI have significance in and of them selves in distinguishing between levels of social adjustment. DeCillis & Orbison (28), in comparing the M-F scale of the MMPI with the Terman-Miles M-F test, obtain results which show that while both tests clearly differentiate between the sexes, they are apparently measuring somewhat different aspects of masculinity-femininity interest and attitude. Fassett (38) found that a group of veterans scored higher on the M-F scale than did a group of nonveterans, a situation which is somewhat difficult to explain, unless one heeds Hathaway & Meehl's warning (60) that this scale is somewhat ambiguous.

A number of other studies relate scores on the MMPI to those obtained on other tests. Canning, Harlow & Regelin (22) find little relationship between the Humm-Wadsworth Temperament Scale and the MMPI and Feather (39) relates MMPI scores of normal and maladjusted groups to Kuder scores. An interesting finding is that maladjusted individuals according to the MMPI tend to have high Kuder scores in the artistic, musical, and literary areas. These findings may well be related to those of Spiaggia (99), who finds that art students obtain significantly higher means on D, Pd, I, Pa, Ps, Sc, Hy scales of the MMPI as compared with a randomly selected control group. Lough & Green (71) are able to show some interesting relationships between MMPI scores and Washburne SA inventory scores. This last test which uses the MMPI scores as criteria suggests the high degree of dependability which is now being ascribed to the MMPI.

An interesting note on the variety of uses to which the MMPI is being put is contributed by Weisgerber & Weisgerber (109), who find no difference in average MMPI profiles for students rated distinctly above average and

those below average in practical nursing, and by Wiener & Werner (111), who find that profiles of a group of 26 veterans planning on becoming embalmers are like those of other people who do not have such plans.

McQuitty (78) describes a unique procedure for deriving what he calls "diversity indices" from test items of the sort that are usually found in the more objective personality tests. He obtains these "diversity indices" by comparing and intercomparing the answers given to successive pairs of items by a group of normal persons and by a group of mental hospital patients. On this basis, he constructs a scale which allocates paired categories of people according to the degree of similarity-diversity which then serves as a standard for allocating individuals. He is then able to demonstrate that so-called "disintegrated personalities" give successive answers which are characteristic of diverse categories of people and "integrated personalities" give successful answers typical of similar categories. This approach offers real hope of producing better predictions from the usual type of personality questionnaire.

Interest and Attitude Measures

There is a continuing interest in the development of new interest and attitude measures and in the further development and application of those already devised. Myers & Shultz (81) described the development of a new attitude-interest questionnaire which contains items covering motivation for college, intellectual interest, teacher relations and study habits, withholding from outside activities, and parental backing. Scoring keys were devised on the basis of responses of underachieving and overachieving groups of students. By including questionnaire scores with two scores from the Scholastic Aptitide Tests of the College Entrance Examination Board increased the multiple correlation from .52 to .53. Since both underachievers and overachievers may represent different types of personality difficulties, it is possible that a scoring key based on differences between underachievers and those who achieved at their predicted level would be more useful.

Cattell and co-workers (24) emphasize the desirability of having more objective measures of strength of attitudes, and interests than are obtained simply by asking a subject how strong these are. Cattell describes 25 different types of procedures for obtaining more objective measurements; they include factors such as amount of income or amount of time a person reports spending on certain courses of action, speed of decision, psychogalvanic reponse, and speed of reading. In the experiments reported, 12 different methods of measuring attitude and strength are applied to 12 different attitudes. Even though the results obtained are not too convincing, the procedures which Cattell suggests appear promising. Strong (102) describes two new scales which he has derived for his vocational interest blank. The first is a Pm or men-in-general scale while the second is a similar Pw scale for women. The information necessary for obtaining and interpreting the Pm score is given in the January, 1951, revision of the Strong Vocational Interest Blank for Men.

XUM

In contrast to the many previous studies which have shown little or no relationships between interest and ability, Wesley, Corey & Stewart (110) report an average correlation of plus .42 between interest and ability based on deviations from individual means when the measures of ability are a series of aptitude and achievement tests logically related to scores on the Kuder Preference Record. The Kuder was also used by Barnette (7) in an investigation of occupational aptitude patterns for engineers. Significant differences on the computational, scientific, persuasive, and clerical scales of the Kuder test were found between groups who followed the agreed-upon vocational plan and were either in school or working and those who dropped or never began work or training. The results in general are in line with those previously reported by Speer (98).

General Intellectual Aptitude and Special Aptitude

A fairly large number of studies relate to tests of general intellectual aptitude and special aptitude. Berdie (14) reports results indicating that the Differential Aptitude Tests, with the exception of the Numerical Ability Test, do not contribute significantly to the prediction of academic success of students of the Institute of Technology at Minnesota. Berdie expresses concern over the fact that prediction correlations resulting from current studies are no higher than, and in some cases not as high as, those reported 10 or 15 years ago. However, he feels that may be due not to the tests themselves but rather to limitations imposed by the criteria.

Pertinent information regarding the use and application of the Wechsler-Bellevue Intelligence Scale is supplied by Kitzinger & Blumberg (68) by means of a supplementary guide for administering and scoring the tests. This guide contains many practical hints. Gerboth (51), who describes practice effects from one complete scale to another resulting from retesting and also from one subtest to another, finds that Form I is more highly correlated with American Council on Education percentiles and with grade point averages than Form II. Seashore (95) cautions against interpreting differences between verbal and performance IQ's on the Wechsler as being due to differences in relative ability. Unusually high correlations, .68 and .69 between Wechsler-Bellevue IQ's and high school grade point ratios, are reported by Frandsen (47). Simon & Levitt (97) present a table showing the relationship between Wechsler-Bellevue IQ scores and occupational area with engineers, professional persons, and educators at the top of the list and skilled labor and personal service workers at the bottom.

The continuing interest in attempting to predict higher level educational success is shown by a study of Travers & Wallace (106), who describe a new Academic Aptitude Test, Graduate Level, which includes measures of vocabulary, reading comprehension, verbal reasoning, quantitative reasoning, and numerical ingenuity. Subtest scores are correlated with five general fields of study which show great differences between the areas of study in the abilities associated with grades. When the subscores of the tests are weighted to give maximum predictions, the correlations range from .17 to .52 when

average grades in the five areas of study are considered. These correlations are considerably higher than those obtained between Miller Analogies Test scores and average grades in the same areas. One wonders whether these differences would hold up if the Miller test were divided into subtests and these weighted optimally for prediction. Mandell (75) attempts the even more difficult task of predicting originality in the physical sciences. A total of 11 tests were administered to the groups studied. Some of these tests yielded significant correlations with ratings of originality; others were able to discriminate between research and nonresearch groups; and two of the tests correlated significantly with ratings on ability in basic research.

Not much that is new has appeared regarding tests of special aptitude. Owens (82) describes an aptitude test for Veterinary Medicine which was administered to 150 subjects in four universites; it yielded a mean tetrachoric correlation between test scores and grades of .62. Mandell (74) describes an "Administrative Judgment Test" developed by the United States Civil Service Commission which yields a mean validity coefficient of .51. There are indications that some of the so-called "primary mental abilities" may not be so primary after all, as Zimmerman & Guilford (120) were able to demonstrate by careful design and by factor analysis procedure that spatial-relations and visualization represent distinct factors.

Anderson (2), in a note on the use of MacAdory and Meier Art Tests in counseling, concludes that, in view of modern art trends, available art tests do not provide an adequate basis for appraising aptitude for fine arts, such as painting and sculpture, and that further research in art aptitudes is greatly needed.

XUM

LITERATURE CITED

1. Ammons, R. B., Butler, M. N., and Herzig, S. A., *J. Applied Psychol.*, **34**, 198–205 (1950)
2. Anderson, R. G., *Educ. Psychol. Measurement*, **11**, 81–86 (1951)
3. A.P.A. Committee on Ethical Standards for Psychology, *Am. Psychol.*, **6**, 57–64 (1951)
4. A.P.A. Committee on Ethical Standards for Psychology, *Am. Psychol.*, **5**, 620–26 (1950)
5. A.P.A. Committee on Training in Clinical Psychology, *Am. Psychol.*, **5**, 594–609 (1950)
6. Barahal, G. D., Brammer, L. M., and Shostrom, E. L., *J. Consulting Psychol.*, **14**, 256–60 (1950)
7. Barnette, W. L., Jr., *Educ. Psychol. Measurement*, **11**, 51–66 (1951)
8. Barnette, W. L., Jr., *J. Applied Psychol.*, **34**, 399–405 (1950)
9. Barry, J. R., *J. Abnormal Social Psychol.*, **45**, 647–58 (1950)
10. Bartlett, M. R., *J. Consulting Psychol.*, **14**, 393–94 (1950)
11. Bellak, L., Levinger, L., and Lipsky, E., *J. Clin. Psychol.*, **6**, 295–97 (1950)
12. Benjamins, J., *J. Abnormal Social Psychol.*, **45**, 473–80 (1950)
13. Berdie, R. F., in *Ann. Rev. Psychology*, **1**, 255–66 (1950)
14. Berdie, R. F., *J. Educ. Psychol.*, **42**, 114–23 (1950)
15. Blaesser, W. W., and Froelich, C. P., *Educ. Psychol. Measurement*, **10**, 588–95 (1950)
16. Blau, T. H., *J. Clin. Psychol.*, **6**, 403–6 (1950)
17. Blum, M. L., and Balinsky, B., *Counseling and Psychotherapy* (Prentice-Hall, Inc., New York, 586 pp., 1951)
18. Bordin, E. S., in *Trends in Student Personnel Work*, 120–29 (Williamson, E. G., Ed., Univ. of Minnesota Press, Minneapolis, 417 pp., 1949)
19. Bordin, E. S., in *Trends in Student Personnel Work*, 105–19 (Williamson, E. G., Ed., Univ. of Minnesota Press, Minneapolis, 417 pp., 1949)

19a. Bruner, J. S., and Postman, L., in *Current Trends in Social Psychology*, 71–118 (Dennis, W. *et al.*, Eds., Univ. of Pittsburgh Press, Pittsburgh, Penn., 1948)

20. Calvin, J. S., and Ward, L. C., *J. Clin. Psychol.*, **6**, 377–81 (1950)
21. Cameron, D. E., *General Psychotherapy* (Grune & Stratton, Inc., New York, 304 pp., 1950)
22. Canning, W., Harlow, G., and Regelin, C., *J. Consulting Psychol.*, **14**, 414–15 (1950)
23. Carp, F. M., *J. Consulting Psychol.*, **14**, 268–75 (1950)
24. Cattell, R. B., Heist, A. B., Heist, P. A., and Stewart, R. G., *Educ. Psychol. Measurement*, **10**, 224–48 (1950)
25. Coffee, H., Freedman, M., Leary, T., and Orsorio, A., *J. Social Issues*, **6**, 1–65 (1950)
26. Cole, D., and Roberts, E., *J. Clin. Psychol.*, **6**, 381–86 (1950)
27. Darley, J. G., and Anderson, G. V., in *Educational Measurement*, 68–84 (Lindquist, E. F., Ed., Am. Council on Education, Washington, D. C., 1950)
28. DeCillis, O. E., and Orbison, W. D., *J. Applied Psychol.*, **34**, 338–42 (1950)
29. Dollard, J., and Miller, N. E., *Personality and Psychotherapy* (McGraw-Hill Book Co., New York, 488 pp., 1950)
30. Dörken, H., Jr., *Am. J. Orthopsychiat.*, **20**, 828–33 (1950)

31. Dressel, P. L., and Matteson, R. W., *Educ. Psychol. Measurement*, **10**, 693–706 (1950)
32. Dubrovner, R. J., Lackum, J. V., and Jost, H., *J. Clin. Psychol.*, **6**, 331–36 (1950)
33. Dugan, W. E., *Occupations*, **29**, 341–44 (1951)
34. Dymond, R. F., *J. Consulting Psychol.*, **14**, 343–50 (1950)
35. Elton, C. F., *Educ. Psychol. Measurement*, **10**, 728–39 (1950)
36. Eron, L. D., Terry, D., and Callahan, R., *J. Consulting Psychol.*, **14**, 473–81 (1950)
37. Eron, L. D., and Ritter, A. M., *J. Consulting Psychol.*, **15**, 55–61 (1951)
38. Fassett, K. K., *Educ. Psychol. Measurement*, **10**, 338–41 (1950)
39. Feather, D. B., *J. Social Psychol.*, **32**, 71–78 (1950)
40. Fein, L. G., *J. Clin. Psychol.*, **6**, 248–53 (1950)
41. Fiedler, F. E., *J. Consulting Psychol.*, **14**, 239–45 (1950)
42. Fiedler, F. E., *J. Consulting Psychol.*, **14**, 436–45 (1950)
43. Fiedler, F. E., *J. Consulting Psychol.*, **15**, 32–38 (1951)
44. Fiedler, F. E., *J. Clin. Psychol.*, **7**, 101–7 (1951)
45. Fisher, V. E., *The Meaning and Practice of Psychotherapy* (The Macmillan Co., New York, 411 pp., 1950)
46. Fosberg, I. A., *J. Consulting Psychol.*, **15**, 39–44 (1951)
47. Frandsen, A. N., *J. Applied Psychol.*, **34**, 406–11 (1950)
48. Friedenberg, E. Z., *Educ. Psychol. Measurement*, **10**, 545–68 (1950)
49. Galdston, I., *et al.*, *Am. J. Psychother.*, **4**, 419–31 (1950)
50. Gandet, F. J., Carli, R. A., and Dennegar, L. S., *J. Applied Psychol.*, **34**, 347–50 (1950)
51. Gerboth, R., *J. Consulting Psychol.*, **14**, 365–70 (1950)
52. Gilbert, W. M., *Educ. Psychol. Measurement*, **10**, 521–30 (1950)
53. Gordon, I. J., *Educ. Psychol. Measurement*, **10**, 505–12 (1950)
54. Gorlow, L., *Nondirective Group Psychotherapy: An Analysis of the Behavior of Members as Therapists* (Doctoral thesis, Teachers College, Columbia Univ., New York, 1950)
55. Gough, H. G., *J. Consulting Psychol.*, **14**, 408–13 (1950)
56. Guertin, W. H., *J. Clin. Psychol.*, **7**, 163–66 (1951)
57. Guertin, W. H., *J. Consulting Psychol.*, **14**, 404–7 (1950)
58. Haigh, G., and Kell, B. L., *J. Abnormal Social Psychol.*, **45**, 659–66 (1950)
59. Harrower, M. R., *J. Clin. Psychol.*, **6**, 213–33 (1950)
60. Hathaway, S. R., and Meehl, P. E., *An Atlas for the Clinical Use of the MMPI* (The Univ. of Minnesota Press, Minneapolis, 799 pp., 1951)
61. Hobbs, N., in *Client-Centered Therapy*, 278–319 (Rogers, C. R., Ed., Houghton Mifflin Co., Boston, Mass., 1951)
62. Holtzman, W. H., *J. Clin. Psychol.*, **6**, 343–47 (1950)
63. Holtzman, W. H., *J. Clin. Psychol.*, **6**, 348–51 (1950)
64. Jager, H. A., *Occupations*, **29**, 252–56 (1951)
65. Kamm, R. B., *Educ. Psychol. Measurement*, **10**, 537–44 (1950)
66. Kaplan, G., *Mental Hyg.*, **35**, 41–50 (1951)
67. Kelly, E. L., and Fiske, D. W., *Am. Psychologist*, **5**, 395–406 (1950)
68. Kitzinger, H., and Blumberg, E., *Psychol. Monographs*, **65**(319), 1–20 (1951)
69. Korner, A. F., *J. Abnormal Social Psychol.*, **45**, 619–627 (1950)
70. Lindner, R. M., *Am. J. Psychother.*, **6**, 432–42 (1950)
71. Lough, O. M., and Green, M. E., *J. Social Psychol.*, **32**, 23–30 (1950)

XUM

72. Luchins, A. S., *J. Personality*, **19**, 64–84 (1950)
73. Luft, J., *J. Abnormal Social Psychol.*, **45**, 756–60 (1950)
74. Mandell, M. M., *J. Applied Psychol.*, **34**, 145–47 (1950)
75. Mandell, M. M., *Edu. Psychol. Measurment*, **10**, 380–85 (1950)
76. Meer, B., and Singer, J. L., *J. Consulting Psychol.*, **14**, 482–84 (1950)
77. Meltzer, H., *J. Personality*, **19**, 48–63 (1950)
78. McQuitty, L. L., *J. Personality*, **18**, 461–82 (1950)
79. Miller, J., in *Recent Advances in Diagnostic Psychological Testing*, 3–12 (Harrower, M. R., Ed., Charles C Thomas, Springfield, Ill., 120 pp., 1950)
80. Mowrer, O. H., *Learning Theory and Personality Dynamics* (The Ronald Press Co., New York, 776 pp., 1950)
81. Myers, R. C., and Schultz, D. G., *Educ. Psychol. Measurement*, **10**, 654–63 (1950)
82. Owens, W. A., *J. Applied Psychol.*, **34**, 295–99 (1950)
83. Pace, C. R., *Educ. Psychol. Measurement*, **10**, 411–19 (1950)
84. Pepinsky, H. B., *Applied Psychol. Monograph No. 15* (Stanford Univ. Press, Stanford, Cal., 140 pp., 1948)
85. Pepinsky, H. B., *Ann. Rev. Psychol.*, 317–34 (1951)
86. Pierson, G. A., *Occupations*, **29**, 92–94 (1950)
87. Riess, B. F., *J. Abnormal Social Psychol.*, **45**, 700–709 (1950)
88. Robinson, F. P., *Principles and Procedure in Student Counseling* (Harper & Brothers, New York, 321 pp., 1950)
89. Rogers, C. R., Ed., *Client-Centered Therapy* (Houghton Mifflin Co., Boston, 560 pp., 1951)
90. Rothney, J. W., and Roens, B. A., *Guidance of American Youth* (Harvard Univ. Press, Cambridge, Mass., 269 pp., 1950)
91. Sacks, J. M., and Lewin, H. S., *J. Consulting Psychol.*, **14**, 479–81 (1950)
92. Sarason, S. B., *J. Clin. Psychol.*, **6**, 387–92 (1950)
93. Schneider, L. I., *Psychol. Bull.*, **47**, 493–507 (1950)
94. Schofield, W., *J. Clin. Psychol.*, **6**, 234–36 (1950)
95. Seashore, H. G., *J. Consulting Psychol.*, **15**, 62–67 (1951)
96. Shoben, E. J., Jr., *Psychol. Bull.*, **46**, 366–92 (1949)
97. Simon, L. M., and Levitt, E. A., *Occupations*, **29**, 23–25 (1950)
98. Speer, G. S., *J. Eng. Education*, **25**, 357–63 (1948)
99. Spiaggia, M., *Educ. Psychol. Measurement*, **10**, 285–93 (1950)
100. Stevenson, W., *J. Mental Sci.*, **85**, 185–205 (1939)
101. Strang, R., *Educ. Psychol. Measurement*, **10**, 531–36 (1950)
102. Strong, E. K., Jr., *J. Applied Psychol.*, **35**, 50–56 (1951)
103. Stuit, D. B., *Ann. Rev. Psychol.*, **2**, 305–16 (1951)
104. Thompson, C. W., and Bradway, K., *J. Consulting Psychol.*, **14**, 321–23 (1950)
105. Thorne, F. C., *Principles of Personality Counseling* (J. Clin. Psychol., Brandon, Vt., 491 pp., 1950)
106. Travers, R. M. W., and Wallace, W. L., *Educ. Psychol. Measurement*, **10**, 371–79 (1950)
107. Tucker, J. E., *J. Consulting Psychol.*, **14**, 283–86 (1950)
108. Tupes, E. C., *Psychol. Monographs* **64**(317), 1–24 (1950)
109. Weisgerber, C. A., and Weisgerber, S. J., *J. Social Psychol.*, **33**, 3–11 (1951)
110. Wesley, S. M., Corey, D. Q., and Stewart, B. M., *J. Applied Psychol.*, **34**, 193–97 (1950)
111. Wiener, D. N., and Werner, S., *J. Applied Psychol.*, **34**, 391–93 (1950)

112. Wiener, D. N., *J. Abnormal Social Psychol.*, **46**, 3–9 (1951)
113. Williamson, E. G., *Occupations*, **29**, 182–89 (1950)
114. Williamson, E. G., and Bordin, E. S., *Readings in Modern Methods of Counseling*, 456–73 (Brayfield, A. H., Ed., Appleton-Century-Crofts, Inc., New York, 526 pp., 1950)
115. Winne, J. F., *J. Clin. Psychol.*, **7**, 117–22 (1951)
116. Wittenborn, J. R., *J. Consulting Psychol.*, **14**, 261–67 (1950)
117. Wittenborn, J. R., and Eron, L. D., *J. Consulting Psychol.*, **15**, 45–50 (1951)
118. Wrenn, C. G., *Student Personnel Work in College* (The Ronald Press Co., New York, 589 pp., 1951)
119. Wrenn, C. G., *Occupations*, **29**, 414–19 (1951)
120. Zimmerman, W. S., and Guilford, J. P., *Educ. Psychol. Measurement*, **10**, 187–213 (1950)

XUM

EDUCATIONAL PSYCHOLOGY[1]

By John K. G. Elmgren
Psychological Laboratory, University of Gothenburg, Gothenburg, Sweden

The learning process and its scientific study is of paramount interest to education and educational psychology. The recent trend in this important domain seems to be a more clear understanding of the facts and their relative importance and a critical scepticism towards learning theories, including attempts to explain the psychological facts gained by experiments in terms of a somewhat doubtful neurophysiology, e.g., brain fields or something else which will be some rather unknown physiological variable.

The need for a meaningful theory of learning that really influences the teacher in her educational work with children in school still persists, and as a matter of fact, very little has been done in comparison with the tremendous work on learning problems with rats in labyrinths, guinea-pigs, primates, and so forth. Experimental learning psychologists in the United States as elsewhere have almost completely neglected the study of children's learning in the school situation.

The child study in general is another important field of direct practical significance to educational work in the school situation. The development of children is much easier to study in the preschool age than at later periods of life simply for the reason that the events in the developmental cycle of children are much more condensed within comparatively briefer time intervals in proportion to the precocity of the developmental units of the total life span, as Gesell (1) has pointed out so ably. The great value of the comparative approach, which has brought so much new insight into the child development up to 18 months in determining the common levels of the child and the primate (cf. Kellogg & Kellogg), has no counterpart in the study of children after that age. The study of the individual differences in childhood as well as in the life of the adult seems to be of a steadily growing interest in educational quarters. Educators have realized that children are not alike in their mental make-up, that they differ from the adult in most respects, and that they must be educationally treated by taking such differences into account. The teacher and the parent must be familiar with the psychology of the child in order to understand its problems and developmental needs.

Much research has been devoted to child psychology, but comparatively little scientific effort has been made to get a deeper insight into the mental life of the teacher. There seems now, however, to be a more and more clear understanding of the fact that education is taking place in a social field and that the interrelations between the teacher and the children in her class must be thoroughly investigated. Modern educational life is so institutionalized and to such a degree characterized by social aspects that the problems of this kind naturally present the greatest interest to educational, psychological, and sociological research.

Character development, habit formation, and the emotional life are of much

[1] The survey of the literature to which this review pertains was completed about March, 1951.

greater importance to education in practice and theory than intellectual viewpoints. Education in itself is a process determined by strong emotional drives, urges, emotions, sentiments, and so forth. For that reason, the three areas (*a*) guidance, mental hygiene, and other practical applications, (*b*) psychosomatic medicine, and (*c*) individual and group therapy form border-line fields having contiguity with education, psychology, and medicine.

Child Study and the Educational Program

In accordance with that view and the recent trends in psychosomatic medicine, for example, no sharp distinction can be made between the somatic and the psychological development of the child. The study of the body type is of great psychological interest, especially the interrelationships between somatic and psychic components as stressed by Sheldon, Conrad, and Kretschmer in works on the human constitution. A clear understanding of the adult constitutional interrelationships necessitates a thorough investigation of children's body form and mental make-up.

Meredith & Culp (2) seem to have realized this is an attempt to utilize different index measurements in order to obtain valid ratios for a description of continua of the slender-to-stocky variety. The authors take as a measure of the amount of over-all rotundity the ratio cube-root of weight/stature, instead of the weight/stature index. This agrees with the biological truth that there is a decrease in stockiness with age. The application of the weight/stature index on the contrary gives a series of increasing values with age which is apparently paradoxical. The ratio weight/stature is to be discarded for the reason that it uses a measurement of mass divided by a linear measurement. In order to get biologically sound and meaningful results the same form of measurement data must be used in numerator and denominator of the ratio. An area must be divided by an area, a volume by a volume and a linear measure by a linear measure. In the present case, adjustment to approximate the same form can be made by taking the cubic root of the weight on the cube of the stature. In addition to the over-all index, three indices of a more specialized range are used by Meredith & Culp:

$$\text{Upper limb index} = \frac{\text{arm girth}}{\text{upper limb length}} \times 100$$

$$\text{Lower limb index} = \frac{\text{leg girth}}{\text{lower limb length}} \times 100$$

$$\text{Stem index} = \frac{\text{chest girth}}{\text{stem length}} \times 100$$

The groups used by Meredith & Culp consisted of two samples of male subjects, one drawn in Iowa City, Iowa, and the other in Eugene, Oregon. Anthropometric data were collected on the former over the years 1937 through 1949 and on the latter between January and May, 1950. The first sample composed of 180 boys residing in or near Iowa City was rather upward selection, the fathers being in 70 to 80 per cent professional men (mainly members of the University faculty) and the remainder,

XUM

business proprietors or managers. In the other sample, composed of 109 boys residing in or near Eugene, from 20 to 25 per cent of the fathers belonged to professional and managerial groups, while the remainder were skilled to unskilled employees.

Some correlation findings reported indicate r's somewhat lower at ages four and five years (.27 to .40) than at ages seven and eight years (.44 to .66). The highest coefficients are for the upper and lower limb indices correlated with the over-all index. The r's at ages four and five years (.63 to .77) are somewhat lower than those at seven and eight (.83 to .86).

Bayley (3) provides a study in which comparisons are made in masculinity and femininity in several characteristics using the M-F scores on the Kuder Interest Record and correlating them with ratings of body form. The sample consisted of 79 boys and 83 girls from two of the Institute of Child Welfare Studies, the Guidance Study and the Berkeley Growth Study. Measures of somatic androgyny were derived from the standards of the Institute's earlier studies. Measures of this kind are based on the independent ratings of three raters, made from body photographs. The girls were grouped into: 12 Hyperfeminine, 37 Feminine, 17 Hypofeminine, 8 Hypobisexual, and 10 Bisexual. The boys were grouped into: 10 Bisexual, 5 Hypobisexual, 10 Hypomasculine, 42 Masculine, and 12 Hypermasculine. These 10 androgyny groups were compared for several different mental and physical traits. Intelligence showed no relation to sex variables, either physical or mental. On the Kuder M-F scale, some tendency was manifested for both boys and girls whose builds were not completely sex-appropriate to have interest scores which deviated in the masculine direction from the average interests of their own sex. This is most marked for the hypomasculine and bisexual boys and for the girls whose builds have masculine components. The hypodeveloped individuals in each sex show some (insignificant) tendency to be taller than their sex average. The general physical trend is in the direction of muscles and strength going with greater masculinity; fat and a "delicate" skeletal frame go with greater femininity. Intelligence showed no relation to sex variables. In general, there is but little interrelation between the masculine-feminine variable in physique and interests, the outstanding fact being that the two series of variables are largely independent of each other.

Frank (4) discusses the concept of maturity and the process of maturation from a dynamic and biological point of view. The human organism is never static. From the moment of conception, all through life, it is changing, and each investigator in accordance with professional training and experience sees in the changing child a different set of problems to be investigated. The various aspects of the organism—personality (structural—functional and behavioral) are selected for observation and measurement. It may, however, be recognized in accordance with the principle of biological relativity, every observation and measurement is relative to the specific organism—personality and the field in which it is functioning. Growth and development are products of the dynamic processes of living organisms. Successive transformations of structure, function, behavior, and emotions are taking place all through life with continually changing patterns and dimensions. Immaturity is best explained as the persistence of earlier patterns.

Krogman (5) points out that maturity is a state rather than a process. The different meanings of normal maturity are discussed in the light of morphological changes, e.g., skeleton advancement. The use of the skeleton is regarded as a basic technique in determining morphological maturity. Quantitative changes must be distinguished from qualitative, however. The skeleton is correlated with growth in

size, but the qualitative features are no less important and constitute the real progress towards an ultimate condition, or the adult level.

Morphological maturity can be related to the growth process irrespective of any correlation with skeletal growth. The most commonly used estimates of morphological maturity are discussed as, for example, Scammon's patterns of growth (neural, lymphoid, general, genital) and Wetzler's patterns of maturity as the percentage level reached with respect to ultimate level.

The concept of puberty is itself a measure of morphological maturity, especially the onset of the so-called secondary sex traits. It has been discussed along lines determined by new findings, e.g., those of the studies of Rechey Reynolds and Shuttleworth & Todd on the process of sexual maturation in boys and girls, where different criteria are discussed as maturity-groups and ossification centers as measures of maturity. It is concluded that maturity is an end product and maturation a process related to inherent impulse to growth. The process of maturation is always going on in human tissues leading to the state of maturity. The process of growth is maturation, wherever it occurs in the life-cycle.

Greulich (6) claims that an x-ray film of the hand and wrist seems to meet the demand for an indicator of maturity which, within reasonable limits, is independent of bodily size. Several cases of precocity depending on various conditions, e.g., gonadal and hypogenital states, are reported. In normal girls, a rather constant relationship seems to exist between the time of occurrence of the first menstruation and the time at which the maximum annual increment in height takes place. Observations are reported indicating that by the time the maturation of the ovaries and of the other reproductive organs has attained a level at which menstruation is possible, the rate of growth in stature has already begun to decelerate. In general, it seems possible to make a distinction by using the skeletal status of the hand as revealed by the x-ray film as a prognostic criterion between children who will mature early and those in whom maturation will be longer delayed. Also, in normal boys, a close relationship is demonstrated between the developmental status of the reproductive and skeletal systems as disclosed by an x-ray film of the hand and the wrist.

Wechsler (7), in connection with the recent standardization of the Wechsler Intelligence Scale for Children, with the equivalent standard score of 15 years 4 months to 15 years 7 months as a base of comparison, gives a survey of the development of different abilities in children from the ages of 5 to 15 years. Many overlapping intellectual abilities are involved in the scale. General characteristics of the curves for most intellectual abilities seem to resemble the growth curve for vocabulary characterized by a rapid rise in the first years followed by a gradual slackening as age 15 is approached. From the point of view of maturation, it is important to note that the curves have different rates of increase and that most abilities increase up to age 15 but at progressively decreasing rates. The pedagogical consequences of the results are discussed, and the author seems to be inclined to believe that educators have systematically underestimated the abilities of children and have tended to ascribe to lack of capacity what is really dependent on inadequacy of the teaching methods.

Jones & Bayley (8) have investigated two groups of 16 early- and 16 late-maturing boys taken from a normal distribution of 90 cases. The physically

accelerated and the physically retarded boys were of the same age but separated by about two years in skeletal age. The boys who matured late were relatively very small from 13 to 15 years. They were slender-built and long legged, and the strength tests showed them to be relatively weak while they were lagging in size. The early maturing group was usually large, broad-built, and strong with the greatest superiority in strength and physical skill at ages 13 to 15. Independent ratings of three staff members showed differences in physical attractiveness. The early maturing in boys is associated with mesomorphy and physical qualities that are rated as attractive by adults. The difference between the two groups in physical attractiveness is most marked at ages 15 and 16. A group of traits which, according to these investigations, also has developmental significance consists of those related to expressiveness. The late-maturing seem to be consistently above the average in this respect. This may depend on a reaction formative to inferiority. The "active small boy" not only expresses through his activity an immature culture pattern but compensates for a less favored physical status.

Riesch (9) collected various data in 20 school units composed of 22 teachers in 12 rural, 6 state graded, and 2 city schools located in southeastern Wisconsin. The Wisconsin Adaptation M-Blank was used to rate the teachers and several tests (e.g., Washburn Social-Adjustment Inventory, California test of mental maturity) were used to determine the degree of social and emotional adjustment of each pupil. It was found that 10 per cent of the state graded, 2 per cent of the city, and 11 per cent of the rural pupils were maladjusted at the time of the pretest. Gain in achievement was found to be positively and significantly related to the pretest conduct. An interesting fact was that factors operating over the three-month period of the investigations tended to increase the maladjustment.

Meredith & Meredith (10) obtained 10 measurements by anthropometric technique on groups of girls and boys of the ages four to eight (about 100 individuals in each group). The annual increments were computed by determining five normative categories of increments, by arranging each series of increments in order from the highest value (largest gain) to the lowest (smallest gain) and employing the thirtieth and seventieth percentiles as the limiting values for the normative category designated "average gain," the tenth percentile as the lower limits of the category "moderately small gain" and so forth. The advantage of this method is ably demonstrated with respect to the clinical significance: A child who shows a discrepancy between moderately large gains in the skeletal measures (stature and hip width) and moderately small gains in body weight and the soft tissues of the arm and leg may be medically examined in order to determine whether the norms reflect the influence of some disease condition, e.g., nutritional need or activity deficiency.

Bennett (11) describes a quick and efficient audiometric test designed to detect, by examining groups of children, partial deafness which is too often wrongly classified as dullness. It was thought that the most thorough speech

audiometric test would be a speech test built up to a phonetically balanced scheme, incorporating the entire range of pitch required for accurate hearing. In the construction of the test, the phonetic scheme used in the Psycho-Acoustic Laboratory at Harvard in testing communication systems served as a model. A group of 500 six-year-old children were tested. Fifty-six children, or 11 per cent of the total group, had losses greater than 15 db. Only four were known to be partially deaf by their teachers.

Siegel (12) reports an experiment with 60 children (20 in each of the age groups seven, nine, and eleven) selected from the public schools of East Chicago. Five tests, composed of toy objects and pictures and word names of these objects, were used. The task for the subject consisted in grouping the items in each test on the basis of belongingness. Results demonstrate similarities and differences between the age groups studied in approach to each test and the developmental trend.

Glassey & Week (13), in a very useful book, deal with important educational problems in a realistic way. Cumulative record-making and the techniques of such procedures are explained and discussed. The outstanding feature of this book is that it is not written for specialist but for the ordinary teacher in order to help him to formulate a judgment of a child's personality. Special aptitudes, interests, disposition, and attainments are discussed, and the book is provided with excellent appendices giving sample record cards, fitted in and explained by case histories.

Zur Nieden (14) reports a follow-up study of adopted children in Germany after World War I. The significance of environment for intellectual development is clearly recognized and ascribed to the special care of the adopting parents. There is no doubt of an influence of the environment on the intellective level of the adopted children. The development was stated by the adopting parents to be bad only in 9 per cent. The fact should be emphasized that, of 29 adopted children whose fathers and mothers had shown antisocial behavior, only four had developed in a socially undesirable fashion. The favorable results are supposed to depend on the special care of the adopting parents, who, in general, tend to stimulate the development of the children more than physical parents do.

Château (15) discusses the role and the nature of childhood in a general treatise on the developmental significance of certain changes. The child growing up under modern civilized conditions does not reflect the successive developmental stages of the race in accordance with a rigid biogenetic law. A simplification is the rule, and the environment forces itself upon the child. Himmelweit & Petrie (16), in a highly interesting study, compared 41 neurotic and 47 normal children matched for age, sex, and intelligence quotient (IQ). The aim of these researches was to build up a battery of personality tests permitting an operational description of the personality of the neurotic child. The group of normal children was used as a control and the tests were designed to assess reactions with regard to specific areas of insecurity, reactions of frustration, and the child's attitudes towards him-

XUM

self and his environment. Thus, it was hoped to get a deeper insight into the maladjustment of the child. Various objective and projective tests were used. Of these, differentiation between the two groups was evidenced at the 0.1 per cent level of significance by a verbal provocation test and a sentence completion test; at the 1 per cent level, by a picture frustration test; and at the 5 per cent level, by a test of static ataxia, a word likes/dislikes test, an endurance test with tasks, a plodding test, and a suggestibility test.

Fourteen tests were used to determine the productive value of a selection procedure in computing by the methods of Penrose (based on a "size" and "shape" score for each person), the multiple correlation between the test scores, and the normal/neurotic dichotomy. This correlation was found to be .73.

Morris & Nicholas (17) tested 46 children and adolescents categorized as primary behavior disorder by the Rorschach technique completed by a specially devised scale. The parents were studied by means of the same rating scale, including 12 essentially intellectual attributes: e.g., paucity versus productivity of associations; emphasis on generalization in approaching problem-solving and on thinking defined as a tendency and ability to attend to the more obvious details of the problem; and rigidity versus refinement of intellectual control and originality of thinking. Seven emotional variables included *inter alia* the following traits: personality type (defined as the balance between extratensive and introversive tendencies), level of maturity of the phantasy life, emotional balance versus imbalance, depth of the emotional life, and sensitivity and tact in reacting to environmental stimuli.

The general results indicate that, in 15 of the 20 variables, the patients are similar to both of the parents. In one variable, the rating on "Overall Pathology," the children differ significantly from both parents, owing to a cumulative reinforcement effect in the personality development of children by parents possessing pathological personality traits. It is concluded that personality development involves a constellation of factors of which the psychogenic and genogenic are only two. Like all psychological tools, the Rorschach technique can not reflect the total etiology. Some of the problems may be clarified by combining the Rorschach and, for example, the electroencephalograph in comparative research.

Roff (18), in an extensive study, surveys the intrafamily resemblances in personality characteristics. Correlations between fathers or mothers and offspring and between siblings are each around .50 in samples which do not deviate too much statistically from the total population. Consistent differences are not found between father-son, mother-son, father-daughter, and mother-daughter correlations. The correlations on various attitudes are in general higher for interparent correlation than those for any parent-offspring combination. Occupational scales, personality inventory scores, socioeconomic status, and occupational level do not differentiate the family pairings. There is, however, a slight evidence that in certain situations a real

difference does exist. Thus, it seems clear that the resemblance of both sons and daughters to mothers may be higher than that to fathers when children are immature but this difference will not be found at maturity. This may be due to the fact shown by psychoanalytic genetic research that children of either sex in the earliest family-constellation are more dependent on the mother and attached to her unconsciously by strong emotional ties. Daughters seem to resemble both parents more than do sons on measures of attitude and opinion which evidently may be ascribed to a greater parent identification.

Wall & Simson (19) investigated the emotional reactions of adolescent groups to 12 films and analyzed for this purpose the responses of 476 boys and 379 girls aged 13 to 16 years 11 months. Identifications with principal characters as stars in the film were made by about one-third of the adolescent audience. Sex differences were found: for example, a greater emotional sensitivity in girls. Many more girls than boys reported experiences of sadness both immediate and relatively enduring. Films dealing with human themes have a greater influence on the emotional life than do comedies and are also of greater importance in influencing attitudes toward others. In general, adolescent girls and boys seem to discriminate between films which are true to life and those which are not. Experiences of fright are relatively rare, and incidents which adults look upon as serious in this respect are seldom noticed by the audience. The cinema experiences do not make the youngsters feel that their own lives are empty or dull, nor do they make them desire money.

Willgoose (20) reports experimental investigations on groups of secondary school boys (300 individuals) aged 15 to 18 years, junior high school boys (75 individuals) aged 13 to 15 years, and 500 junior and senior high school boys which indicated that the strength index determined by different physical exercises (standing broad jump, 30-yard crab race, and 50-yard dash) is a fair measure of motor co-ordination, especially in activities where gross strength is a factor. By using the Pearson product-moment method, a correlation coefficient of $.47 \pm .065$ was obtained between strength index and speed in the 30-yard crab race.

McGhee (21) used a modification of Thurstone's psychophysical method of equal appearing intervals in order to construct a suitable scale to apply to 185 senior secondary pupils for measuring attitudes to five different topics: films, youth clubs, homework, discipline, and sport. The general mean attitude to all five topics was positive and favorable—the attitudes to clubs being the most favorable of all. The most strongly negative correlation was found between films and intelligence ($-.71$), and as a corollary the most positive attitude to films was achieved by pupils of lowest intelligence. The most negative score to discipline was yielded by those of lowest intelligence.

Carter (22) tried to examine by factor analysis the instructors' ratings of student personality traits in using a rating scale of personality traits (leadership, intelligence, knowledge and skills, responsibility, and co-operation). Three factors were isolated: the first produced loadings for each trait and

XUM

seemed to correspond to "general ability to please instructors" (halo-effect); the second was significantly loaded in intelligence and knowledge; and the third, which was referred to as "cooperative responsibility," was heavily loaded with ratings of responsibility and co-operation. The study seems open to criticism on the basis of the few variables selected and of some weaknesses in the statistical procedure.

Haines (23) points out that Piaget's study on egocentric responses in children has not been corroborated in America by research work in the same field, for example, by McCarthy, who found a much smaller percentage of egocentric responses than that reported by Piaget. One hundred children diagnosed as problem children by a psychiatrist and 50 considered to be normal in their behavior were submitted to the first item on the Action-Agent test of the Merrill-Palmer scale, but no statistically significant difference between the groups was found.

Rautman & Brower (24) tried the Thematic Apperception Test on 468 public school pupils in 1950. The responses were compared with those of a group of 536 children from the same school, tested in 1943 in order to study the frequency of war themes in the stories. Although the proportion of war themes decreased from 6.47 per cent in 1943 to 1.77 per cent in 1950, the proportion of responses involving killing and death remained remarkably constant.

Kates (25) puts forward an hypothesis on the different categories of responses in the Rosenzweig picture frustration test and their relation to suggestibility in children. Thirty-one subjects, ranging in chronological age from 101 to 129 months, were investigated in order to test the hypothesis. The subjects were significantly more submissive to the father than to the mother and the peers. Those submitting to the mother were found to be more extrapunitive. Rejection of the mother was on the contrary related to intropunitiveness. The less suggestible subjects were more extrapunitive and less impunitive than the more suggestible subjects.

Weider & Noller (26) studied children's drawings of a person in order to elucidate the influence of socio-economic level on the awareness of sex differences. Three sharply distinguished levels of socio-economic classes were obtained by using as criteria the average years of schooling, average rental values, the percentage of dwellings with more than 1.5 persons per room, etc. The subjects consisted of 153 children, of which 73 were boys aged 8 to 10 and 80 were girls of the same ages. Statistically significant differences were found at the 1 per cent level of confidence in this sense: 74 per cent of the boys and 97 per cent of the girls drew their own sex first; 52 per cent of the boys and 80 per cent of the girls drew their own sex larger. A statistically reliable increase in characteristics drawn seems to exist from socio-economic level A (upper) to level E (lower), but the number of responses verbalized in interviews did not present the same statistical tendency. The results are discussed with reference to their significance in building of healthy and mature attitudes.

Malrieu (27) has followed the evolution of a certain trend in children's drawings by a study of 500 children from 3 to 14 years of age. The instructions were very simple: "Draw something that you really like, something which has pleased you." In the ages three to six years, the child expresses his emotion in a very schematic way, but with a certain tendency towards esthetic rhythm, especially manifested in the repetition of an identical motif. From the age of seven the child's drawings grow much more concrete and detailed. The child draws what he knows about things in a realistic manner and realizes that drawings are not the products of an imaginative play but a real representation or definition of objects. However, the animistic bent often predominates. The sun for example is represented by a human profile, a roof is drawn as a hat, etc. At the age of nine, children begin to insert objects in a frame and to depict real scenes. The most important change takes place about the age of 10, when children manifest a tendency to express their own actions on the external world and form their concepts of life and reality. At the beginning of the period of puberty, a contrary tendency makes the child adapt himself to the adults and express more stable sentiments, which should be investigated also for teaching purposes.

Broida and co-workers (28), in using a modified form of a new projective test, the Symonds Picture-Story Test, made a study of the reactions of 30 crippled children divided into three groups: (*a*) a group where the need to be free from feelings of fear was overmet, (*b*) a group normal with respect to this need, and (*c*) a group where the need was not being met. Statistically significant differences between these groups are discussed; e.g., the second group had a higher number of morality themes and produced almost five times as many guilt themes as the first group. Of major interest is the finding that with the presence of feelings of fear is coupled a desire for social participation. The need for well-planned guidance and counseling services for crippled children is stressed.

Trankell (29), in an extensive investigation, deals with left-handedness and the technique for measuring this trait. A scale is presented which consists of two parts, the impulse-scale and the assymmetry tests adapted to the two main aspects of left-handedness, the preferential hand-usages and the relative skill and dexterity of the two hands. The writing-hand problem is treated in a follow-up study of 746 schoolbeginners during the first six months. The scores on the impulse scale are associated with the children's ability to learn to write with the right hand. It is also shown that social adjustment has an influence on the left-handed child's ability to learn right-hand writing, the more well-adjusted child succeeding in gaining the new habit of right-hand writing. The self-confidence level of left-handed children generally rises when they have established their right-hand performance, the opposite being the case of those who fail. The failing children, however, regain most of what they have lost in confidence when allowed to go back to left-hand writing. The relationships between handedness and other traits are discussed on the basis of factorial results gained on 102

XUM

children, 51 of which were left-handed and 51 right-handed. A new and rather interesting hypothesis on the heredity of handedness is put forward. According to this hypothesis, which certainly needs a fuller description in details and further verification as the author himself points out, right-handedness is caused by a disposition which is inherited as a Mendelian dominant factor. When this dominant factor is present, double (RR) or simple (R), right-handedness results, but the absence of this factor does not predestinate for left-handedness because the recessive genes (rr) are not the only condition for left-handedness; external factors have a great influence on making homozygotic recessives phenotypically right-handed.

Doll & Walker (30) have studied 34 cerebral palsied children with a diagnostic scale of left-handedness. The subjects ranged in ages from 4.0 to 19.5 years and displayed various disabilities due to birth injuries, presumptive birth injury, mongoloid retardedness, and feeble-mindedness. Practically all these children suffered from language disabilities, e.g., severe or moderate speech and reading handicaps. The handedness was diagnosed very carefully in order to detect the natural preference or native dominance. It is stressed that the child should be led to accept one hand or the other. When the injury is on the dominant side, the child should be encouraged to carry through the shift. Speech and other educative processes are forwarded when the disestablishment in handedness is resolved by either re-establishment or transfer, but in general the child must accept one hand as dominant. Critical diagnosis and special training help to overcome the educational difficulties of handedness in palsied children, but it is only one of many factors in this complex problem.

Lowenfeld (31) presents her world test and is especially anxious to stress that this technique must be used for the purpose it was constructed. The apparatus and material as well as the presentation of it are discussed. The author stresses the fact that the world material, apart from the single case of mentally defective and very disturbed children, does not at the first presentation give very many valuable results. It is recommended that the kind of material (people, animals, houses, trees, and so forth) be varied according to national differences in the countries where it is applied. The main thing is to construct the tray and the other parts of the apparatus in such a way that the free hand activity of the child is not hindered. The reactions are evaluated with respect to the significance of the way in which the particular individual structures his own sensual and proprioceptive experience.

In a highly interesting study, Bayer & Snyder (32) have investigated the illness experience of a group of 126 normal children drawn from a larger population selected at random from the Birth Certificate Registry of Berkeley, California, beginning January 1, 1928 and closing June 30, 1929. A group of the same number matched for significant socio-economic factors served as a control group. The experimental group was subjected to different procedures, discussions, counsels, and interviews concerning many aspects

of personal life, including the medical. Medical examinations were made at the Institute of Child Welfare at 21 months, 36 months, and at yearly intervals thereafter through 18 years. The interval health history as well as the current status were noted by the examining physicians. The developmental status and the skeletal maturation were also evaluated.

The main conclusion of this study confirmed the results of earlier researches in the field that a surprising amount of illness in childhood is compatible with the concept of good health. Among the acute communicable diseases, measles, chickenpox, whooping cough, and mumps are responsible for most of the morbidity. Over 70 per cent of children have measles or chickenpox before they are 18, and over one-third have mumps; whereas diphtheria is very rare, and the high incidence figures for whooping cough have been sharply reduced by introduction of active immunization in the last decade. Over 45 per cent of the children reported at least one cold per year. A rather interesting fact is that the highest incidence of colds was between the first and second birthday and from then on there was a rather irregular decline with age. Analysis of the cold experience of the same children over the years gives evidence of differences in natural immunity. Findings are also reported in the domains of allergies, gastrointestinal and abdominal disturbances, and surgery. The boys had more accidents than the girls: 73 per cent of the girls had only one or no accident, but only 51 per cent of the boys escaped with so little injury.

The Bender Visual Motor Gestalt test as a measure of school readiness has been considered by Harriman & Harriman (33), who report results obtained from the administration of this test to two groups of normal children; the first group being 30 children in the Pennsylvania State College Nursery School and the second group being a random selection of 30 children in the second grade in State College, Pennsylvania. Significant differences in at least four major determinants between five-year-old and seven-year-old children are reported and discussed.

Heuyer (34) compares the different contributions of the educationalists, the psychologists, and the child psychologists in the treatment of educationally backward children. The author claims that in France the school psychologists and clinical psychologists neglect too much the psychiatric point of view in the treatment of this category of children. Co-operation is recommended, and the role of the psychologists is clearly recognized as important, especially in the diagnosis of backwardness. Following Binet, the author discusses the advantages and draw-backs of the different methods. The so-called educational diagnosis of mental and educational backwardness is criticized for giving very unsatisfactory results. As a matter of fact, the number of morons in France evaluated by educational diagnosis was 50,000 whereas the more refined diagnosis in 1943 which used a psychological group test revealed the number to be 350,000. Excellent illustrative examples are given, indicating the necessity of a thorough medical diagnosis of mental and educational backwardness. However, the author seems probably to be

XUM

too reluctant against the psychologists' work in the field of mental therapy.

Valentine (35), in a very useful book especially designed for students in educational departments, treats the topics of psychology bearing on educational problems. The general nature of cognitive abilities are discussed as well as learning memory and the need for experimental and statistical research. Avoiding the onesidednesses of different schools of psychology, the author gives a very clear account of individual differences and character. As a child psychologist, the author stresses very strongly the importance for the educationalist to consider the laws of mental development from infancy to adolescence and the needs of special treatment of the backward, the delinquent, and the problem child by appropriate psychological methods.

The psychological research work in connection with the recent reform of the Swedish school system is reported by Elmgren (36). This work included the testing of more than 10,000 children of both sexes, mainly in the school districts of Gothenburg. A considerable part of the investigation was devoted to a comparative study of practical and theoretical aptitude of 6,000 children of the ages 10 to 15, uniformly divided with regard to sex. A modification of the American Minnesota assembly test was used, and results were compared with those of the American Minnesota investigations in 1930. The developmental curve of mechanical aptitude is traced for the above ages and also for supplementary boy groups of the ages 16 to 18.

It seems quite established from the investigations in question that the type of practical aptitude measured by the modified Minnesota assembly test has an extremely complicated course of development connected with the whole course of puberty and that the maximum rate of development for boys within the period dealt with by the investigation occurs between the ages of 15 and 16. If curves of the development of general intelligence are compared with those of the practical type of aptitude, the difference appears clearly. General intelligence develops year after year with continuous increments, which however regularly decrease with increasing biological age. It appears quite otherwise with the practical type of aptitude, which shows every indication of being characterized by a kind of retardation in tempo of development during practically the whole period from the prepuberty stage through the puberty crisis and on to the postpuberty stage, i.e., practically the whole of the age span 12 to 18 years.

Since the so-called "6,000 children" investigation of theoretical and practical aptitude was carried out on relatively equal-sized groups in the ages 10 to 15 years, it is also possible to compare the results of boys and girls in the Swedish material. Significant differences in mechanical aptitude were found to exist, the boys being considerably superior at all age levels (critical ratios in all cases were 14.0 or larger). The differences found between boys and girls are considerably more marked than those shown by the American Minnesota investigations in the 1920's and may probably be thought of as depending mainly on hereditary dispositions in view of the rather steady increase of the critical ratios with age.

By a statistical comparison, it was possible to prove that children in the schools for the backward are much less retarded in the field of practical aptitude than in general intelligence and consequently require practically-biassed vocational education to a much larger extent than is actually foreseen in school education.

In other parts of these investigations, extensive attainment testing is reported, including a great number of factorial studies and also establishment of the maturity of a total of 402 boys and girls in the sixth and eighth classes of the elementary school. The fifth chapter of this report, which deals especially with the linguistic ability of boys and girls, shows *inter alia* that girls mature in this respect at a faster rate than boys during the school-period. In a final chapter, the educational consequences of the total investigations carried out are discussed.

Intelligence and Aptitude Testing; Mental Measurements

Schlesser (37) presents evidence that an average gain of 22 percentiles (68 scaled score points) actually takes place in scholastic aptitude over a 12-week period by five classes in which the average age was 24.5 years. It is claimed that about half of this gain might be accounted for by practice effect, regression, and maturation. At least 11 percentiles or 38 scaled score points may be attributed to the effects of the training program. The consequences for freshman counseling are discussed, and the importance of encouragement seems to be validated accor ding to these researches.

The constancy of the IQ is discussed by Brown (38) by comparing the IQ scores of two levels. In the first grade, 26 children were tested with the Stanford Revision of the Binet scale, and 12 years later, the pupils were retested in the first college year with the Army Alpha or the Otis Higher Examination. The differences are extremely small between the scores of the same individuals, and it is concluded in agreement with Terman and others that the IQ remains constant or relatively constant for each person.

Harris & Schmid (39) suggest a method by which an initial oblique solution, such as that given by the multiple-group method of Thurstone, may be rotated directly to the primary solution without the necessity of calculating an intermediate orthogonal solution. The procedure demonstrated is that of calculating an arbitrary oblique solution for which the number of factors equals the minimum rank in meeting at the same time the demand of reproducing the correlation matrix within the desired limits of sampling error. The method is completed by choosing a new co-ordinate system that gives the desired oblique solution. A fictitious sample is given, and the necessary calculating operations are performed.

Mosier (40) and others, in a special symposium, discuss the need and meaning of cross-validation. The different designs are distinguished; the effects of shrinkage and the necessity of double, cross-validation in order to maximize the prediction criteria are especially stressed; a combination of cross-validation and the statistical-inferential approaches are recommended.

A continuation of the *Abilities of Man* has been prepared by Jones & Spearman (41) in a book appearing after the death of Spearman. This is a very interesting and valuable contribution to the discussion of the theoretical concepts underlying mental

XUM

measurements. The theory of two factors of the late Professor Spearman is presented again and discussed on a partly new basis including new formulas for deduction of g and s. The g-factor, now said to be essentially characterized by the combination of neogenesis with abstractness, is discussed in a chapter devoted to bridging over the factorial gap between statistics and psychology. In a very condensed chapter, the development of factorial methods is discussed and criticized. The spatial representation of factors and the matrix notation are recognized as being full of promises of a great advance in factorial theory and practice. The centroid method of Thurstone and the method of principal components of Hotelling are said to be characterized by the great weakness that every factor except the first produces positive values in about one-half of the tests and in the other half, negative ones. The question is raised whether communalities are justified as stop-gaps. The filling out of the vacant diagonal cells of the correlation matrix by the highest correlation in the column at issue or by the columnar average according to the averoid method of Holzinger increases mathematical tractability but may have a less favorable influence on probability. It is noteworthy, however, that the authors, starting from the set of data presented by Thomson in a matrix of intercorrelations for four tests and comparing different factorial methods (e.g., the "*g*" saturation, the centroid load of Thurstone, the principal conponents of Hotelling, and the different summation methods of Burt) arrive at practically identical values of loads. The minimizing of the number of factors and the maximizing of the number of zeros are discussed as a matter of economy versus significance. The rotational procedure is stated to be arbitrary, but the authors do not completely take issue with the simple structure concept of Thurstone, nor do they consider the case of second order factors in connection with the oblique reference axis. They introduce the concept of "broad" factors as being distinct from group factors, but the distinction seems to lack clarity. An interesting method is presented in the chapter dealing with mechanical factors; the so-called coefficient of excess is based on the distinction between intragroup and intergroup correlations. The method of agreement and differences is the logical principle throughout the book. Important psychological problems also dealt with are attention, fatigue, law of inertia, and oscillation.

In a well-written book, Vernon (42) develops a very consistent view of human abilities as structured or hierarchically divided on different levels. Factors are categories for classification of mental performances rather than entities in the mind. The differences between British and American workers in the field of factor analysis are stressed mainly on the basis of the idea of general ability prevailing among British workers in the field of mental testing and factor analysis. Multiple factor theories in accordance with the views of Thurstone are rejected in favor of what is called "general plus group factor theory." With the introduction of the concept of second-order factors by Thurstone and the acceptance of group factors by the Spearman school, a reconciliation seems established between these originally controversial views. The hierarchical theory seems supported by the great bulk of empirical work that Vernon himself carried through among recruits during the 1939 to 1945 war. The importance of the factors of character and temperament is stressed throughout the book, and a general survey is given of the broad categories of factors. Tests are ascribed to two groups of factors after removing g: the verbal-numerical-educational (v:ed factors) versus the practical-mechanical-spatial-physical (h:m factors). Educational attainments are analyzed in terms of these factors, and it is claimed that school success depends mainly on a combination of the X-factor of character and the verbal-numerical-educational type of factor.

Intelligence is, according to Knehr (43), dependent on the flexibility of behavior or the notion of the complexity or differentiation which can be found in the organism, particularly the elaboration of the central nervous system. In the intact organism, intelligence can be identified with the variety of response mechanism in homeostatic situations leading to modifications of behavior obtainable with the given organism.

Portenier (44) compiled data for students who had received awards or were elected to professional and honorary fraternities at the University of Wyoming during the 20-year period from 1929 to 1949 in order to ascertain to what degree such students excel in tests of mental ability. A positive correlation between psychological test ratings and honor awards seem to exist, but it is also shown that many students who make high test scores fail to win honors.

Feifel (45) administrated the Form L of the Stanford-Binet to 900 children between the ages of 6 and 14. Qualitative differences in response seem correlated with age, the younger children giving significantly more description, inferior explanation, and repetitive types of response in comparison with the older children who more often used the synonym and explanation type of response. The differences originate in different modes of thinking, which are of a more primitive and immature kind in the younger children, strongly resembling those of abnormals.

Lennon (46) states the problem of the relation between the intelligence level of the elementary-school population of a community and the achievement tests. The investigations were performed in connection with the standardization of the new edition of the Metropolitan Achievement Tests and were based on the results for 70 of the 250 communities which participated in the standardization program. There was a marked tendency for communities which are above average IQ to be above average also in achievement as measured by the tests used. The correlations ranged from .11 between IQ and Arithmetic Fundamentals at the second grade level to .86 between IQ and vocabulary score at the eighth grade level. The median value for all correlations was .60.

Belman & Evans (47) designed an experiment in which records of eight tests taken by 27 graduates of the Trade and Industrial Education curriculum at Purdue University were studied. The best individual predictor yielded a correlation of .67 with the criterion. A test battery was also made up of 16 separate predictors and administered to all World War II veterans and 25 per cent of the nonveterans in the Trade and Industrial Education curriculum. The criterion of grade point index after transfer was correlated with each of the predictors. The conclusions were that the scholastic success of students in industrial education can be predicted, and that such predictions can be used in counselling students.

Marks (48) discusses some sampling problems in educational research and gives an illustration of how to draw a sample of the school systems of the United States by grouping them in strata. The probability of selection

within each stratum and the appropriate way of collecting data are discussed. The importance of clear statement of a purpose is especially stressed and certain misconceptions about sampling are treated critically.

Berdie (49) examines the Differential Aptitude Tests Form A as a predictor of engineering training by applying it to 472 freshmen entering the Institute of Technology of the University of Minnesota in September, 1948. These tests, with the exception of the Numerical Ability Test, do not contribute significantly to the prediction of academic success in the Institute of Technology at Minnesota. It is suggested that a version of the Numerical Ability Test which is more difficult than the present forms may prove to be of value for the purpose in question.

Brown (50) has made an experimental approach to the listening process in administering two sections of a test to a heterogenous group of subjects varying from high school juniors to technical candidates aged from 16 to 31. He found that listening ability depends on something besides intelligence but also differs from reading comprehension and scholastic aptitude. Listening is a complex skill that does not seem to improve without special attention.

An occupational aptitude pattern for engineers is presented by Barnette (51) who, in a follow-up, examined a total of 103 successes and 45 failures by computing the mean scores for a variety of tests including the Kuder Preference Record and Iowa Mathematic and Chemistry Aptitude Examinations. The psychometric data are presented in profile from successes to failures. The success group departs significantly from average on six of the nine scales used, the success and failure profiles being rather distinct.

Adkins (52) exposes the principles of observational technique and the evaluation of behavior. Reliability and validity of evaluation are discussed. No distinction can be made between an observational technique as a predictor and as a criterion. The types of summary evaluations are treated in detail, and the useful approaches for improving them are recommended.

Travers (53) makes a distinction between what he calls the technician's approach and the rational hypothesis approach in the field of mental testing. The distinction is exemplified by comparing the approach used in constructing the Strong Vocational Interest Blank with that used by Binet and Simon in the development of their intelligence scale. The advantages of each of the two approaches are referred to. The rather meager advances made in many areas of psychological measurement during the last 20 years are mainly ascribed to the overwhelming influence of the technical interest of producing useful instrument devices and the corresponding lack of deeper understanding of the rational hypothesis approach.

Burt (54), in a symposium on selection for secondary school, presents arguments against selection at the relatively early mental age of 11. Some slight tendency towards either an academic or technical bias is discernible, but it is in evidence at that or even an earlier mental age only in a small proportion of cases. The majority of children must be allocated by a comprehen-

sive scale at a later stage by taking into account, besides innate intelligence and educational attainments, also temperament and health, not to speak of social conditions. The borderline cases must be studied in greater detail, and the psychological considerations must be regarded as limiting factors but not the sole factors in the choice of the most appropriate form of education for each individual child.

Anstey *et al.* (55) in two experiments use a general classification test on two groups of soldiers, 500 in each, in order to determine the interrelations between cultural-educational level and general intelligence. Practically no overlap is reported between children leaving school at 14 and those with more advanced education.

Foulds & Raven (56) report a series of experiments with the progressive matrices of a nonverbal test of intellectual efficiency designed for use with people of more than average intellectual ability drafted in 1943 at War Office Selection Boards. A total of 1,844 testees, including children from the age of 10.5 years to university students, supply the experimental data for this study. Item analysis and retest reliability are presented together with working norms for a total sample of 47.1 children at $10\frac{1}{2}$ years of age and 596 children at $12\frac{1}{2}$ years of age.

Important data on the reading factors in the 1937 revision of the Stanford-Binet intelligence test are presented by Bond & Fay (57). It is clearly demonstrated by these researches that children of the fourth, fifth, and sixth grades whose reading ages are less than their mental ages tend to be penalized by items verbal in nature and that the child with superior reading ability probably tends to be overrated on mental age as measured by the Stanford-Binet. A revised scale is recommended because neither a correction for reading ability nor the mere elimination of verbal items is considered to be a satisfactory solution. Only a restandardization can insure a valid and reliable measure. The conclusions arrived at in this study may be generalized and considered as valid for the whole field of intelligence testing. Verbal items evidently must be controlled in all intelligence test-scales in view of the fact that differences in reading ability may have a favorable or nonfavorable influence on the results.

Kenney (57a), in a survey of 23 group intelligence tests in actual use, computed the quantity of mathematical tasks in percentage of total items and recorded values ranging from 9 to 36 per cent. This considerable variation in amount of mathematical material in tests can be eliminated by determining exactly the relation between what Thurstone calls number facilities and intelligence. Borg (58) submitted a group of 427 art college students to a battery of tests designed to measure mechanical comprehension, ability, and interest. A perceptual factor seems to exist and to be related to artistic ability in the revised Minnesota Paper Form Board series AA and the test of mechanical comprehension form AA and W1. This large perceptual factor is present in art school success and probably in artistic ability. Seashore and co-workers (59) report the results of the standardization of the Wechsler

XUM

Intelligence Scale for Children (WISC). The WISC consists of 12 tests divided into two subgroups identified as Verbal and Performance. The scale was standardized on a sample of 100 boys and 100 girls at each age from five through 15 years. IQ tables were constructed by keeping the standard deviations constant from age to age and comparing the scores earned by individuals in a single age (that is, his or her own) group. The IQ of 100 on the WISC is set equal to the mean total score for each age and the standard deviation in accordance with Terman is set equal to 15 IQ points. Norms are given for different groups of children, and the actual determined percentage of feeble-mindedness and different degrees of intellectual superiority are compared with the expected values. The verbal and performance IQ's on WISC were compared by Seashore (60), who concludes that while the theoretical average subject has a verbal IQ equal to his performance IQ, the individuals as such may present large discrepancies depending on the type of special endowment. This is interestingly shown in grouping the individual results by parental occupation. In group I, the professional parents category, there is a decidedly higher percentage of children with verbal IQ's higher than their performance IQ's.

Achievement Tests and School Learning

Goldman (61) states the hypothesis that cultural patterns in the environment condition the approach to problem-solving in answering intelligence tests. The general capacity for learning must be considered in the light of the influence of emotional factors and cultural differences. The direction of American educational theory is said to be conservative and ethnocentric in the sense of reckoning too much with the individual and his constitution and too little with cultural patterns. As a matter of fact, the educational success or lack of success of a particular student can very often be explained in terms of his cultural background. The author pleads for a better understanding in educational research of the pattern of educational achievement depending on group-formation, and he suggests that this will be a very fruitful line of inquiry. The educational function as a control and direction of the process of communication in the interest of producing intelligent persons is discussed by Jordan (62), who especially stresses the moral aspects of education.

In a very useful and extensive review, Garrett (63) surveys the many studies during the three latest decades devoted to the problem of predicting general scholastic achievement in college by intelligence, aptitude, and achievement tests, combined in certain cases with other criteria, e.g., the marks in high school. It is clearly indicated that the combination intelligence test and the high school average is the best measure of forecasting college success. The median zero-order coefficient of correlation between intelligence and college success was found to be .47, and for high school grade average the median was .56. The median multiple correlation by a combination of these two factors was .62. Anderson (64) selected a sample of representative

high schools in Minnesota by using the method of stratified sampling. The final sample drawn consisted of 56 schools offering both biology and chemistry. These two secondary school sciences were studied on 1,980 students involved in the biological portion and 1,363 in the chemistry portion with the purpose of getting insight into (*a*) the factors which contribute most to achievement, (*b*) the relative contributions of factual information, scientific attitude, and intelligence to the understanding of scientific method, and (*c*) the current practices in the teaching of biology and chemistry. Evidence is given for a revision of the educational program in the fields studied in view of the facts revealed by these investigations.

Furst (65) deals with the relationship between a particular pattern of learning experience and the organization of certain outcomes in general education. In this study, the author makes sure that the objectives measured were desirable for the eleventh and twelfth grades, that the tests were reliable and valid, and that the statistical procedure was sound. In a further study, the same author (66) continued his research by means of factor analysis and tested two hypotheses considered as probably valid for both groups selected. He found that the results for the high school group were defined by a rather broad language art factor. The college group was characterized by a single set of factors largely determined by the different subject fields.

Humphreys (67) makes an attempt to interpret the transfer-of-training literature in psychology in its relation to the problems of general education. The contributions of Judd and the Gestalt psychologists seem most appropriate for the teaching of general principles and the organization of the learning process. Blanchard (68) summarizes the studies in the field of social learning and finds class atmosphere, the personal social relations between students and between students and teachers, to be most important for the skills to be acquired.

Carter (70) investigated the study-methods of five classes in educational psychology, including over 800 students, who were asked to fill out various inventories. The comparison was made between good and poor students by applying two differential statistical procedures based on different weights. Correlations of different size between these and a single mid-term test are reported. Reliability of the study methods inventory was .64, and the average of the two study tests had a reliability of .78, corrected by the Spearman-Brown Prophecy formula.

Alexander (71) tries to determine the goals of curriculum improvement and the organization of the educational environment by school councils, faculty planning, working committees, and so forth. No sharp lines ought to be drawn between pupil, parent, lay and teacher participation in curriculum improvement, and an atmosphere of co-operative planning and evaluation should pervade all groups and the school as a whole. Hopkins (72) makes a case for a more careful consideration of curriculum construction according to psychological principles in the schools. Each child makes his own curriculum

XUM

out of the environment that the school offers. The main function of the curriculum is to mature the child in the direction of developing and enhancing the phenomenal self and to give opportunities for co-operative and self-creative activities.

Lorge & Kruglov (73) found that if educational level is held constant, the structural elements of written expression are not related significantly to measures of intelligence. The expressional level and comprehension may differ for the same person, and for that reason, the quality of the content communicated should be carefully observed by teachers in the classroom.

Patterson (74) compared rural and city schools in Scotland with respect to the effects of pupil-migrancy on ability and achievement. Considering as a migrant a pupil who has made more than one change of school, rural candidates include 28.95 per cent and urban candidates 9.26 per cent of migrants. In both types of school, the mean migrant intelligence quotient is less than that of the mean nonmigrant. In spite of a much heavier incidence of pupil migration, the ratio of performance to ability is greater in the rural schools than in the urban schools.

Ammons & Manahan (75) used their full range picture vocabulary test in testing a representative group of 71 rural children with the 226-item preliminary forms of the test. They compared the final forms A and B of the picture vocabulary test with the Binet vocabulary score. The final forms A and B were found to correlate .88 and .85 with the Binet vocabulary and .92 with each other. The authors conclude that the full-range picture vocabulary test, on the basis of its high reliability and validity, can be recommended for individual testing of the verbal ability of rural school age children for educational or clinical purposes.

Pilliner (76), by using Fisher's Z-technique, pooled a number of observed coefficients in achievement and intelligence testing in order to locate a border-line group in examination. The method of constructing the table is described in detail. The position of the border line, determined by the percentage intake of the grammar schools, can be read off in the table. Richardson (77), using the methods of Thurstone in a valuable study, made a factorial analysis of reading ability in 10-year-old primary school children. A battery of 21 tests and assessments was given to 260 children. Intercorrelations of the 21 tests, assessments, and ratings were computed for boys and girls separately. In general, boys' correlations were found to be more consistent than girls'. Three factors were identified: (*a*) general intelligence as estimated by verbal tests, (*b*) emotional factor of application and responsiveness to the school situation, and (*c*), very tentatively, a specific factor appearing in the tests concerned with the mechanics of reading. The tests of fifty of the most backward readers, irrespective of sex and the cause of backwardness, were submitted to a further factorial analysis which presented some interesting differences from the results obtained in the main analysis. Failure in reading seems to depend on (*a*) specific verbal ability, (*b*) nonverbal intelligence, and (*c*) an emotional factor similar to that which

appeared in the main analysis. The main conclusion is that the two factors of intelligence and verbal ability seem to be of greatest importance as general determinants of reading success or failure. At the same time, in almost every case, school failure was characterized by complete lack of confidence and some emotional disturbance. Specific ability in auditory and visual perception does not emerge as a general differential cause of reading ability at the age of 10 years.

Fitch and co-workers (78), presenting evidence that frequent testing acts as a motivating factor in achievement, recommend, on the basis of their results, instructional supplements in order to make such motivational learning profitable for educational purposes. Moonan (79) has applied the sequential procedure to an achievement examination and describes the technique in detail. A discussion is given of the greatest utility of sequential analysis in sampling data. The main conclusion is that this technique may be used with achievement examinations with some reservations and may also hold some promise for solving other problems.

Downie and associates (80) take issue with the question of arranging a valid program for evaluating the outcomes of general education. The importance of a test of the knowledge associated with general education, tests which give the student a chance to demonstrate his ability to apply what he has learned to new situations, scales which reflect the attitudes and opinions, and other types of evidence are especially brought to the reader's attention. General education is considered to be a process of integration and synthesis. It is recommended that each instructor's course should, as far as possible, be applied to the daily living of the students.

Schultz (81) applied the Medical College Admission Test to medical-college candidates. The selected individuals were male, regular, full-time students who had attended one of five undergraduate colleges. No support was offered for the hypothesis that taking additional courses in biology, chemistry, and physics beyond a certain minimal number leads to better scores on the Medical Science Admission Science Test.

Eames (82) states the relationship of reading and speech difficulties in anatomical, physiological, and psychological terms. Neurological lesions may impair both speech and reading. Emotional reactions to speech difficulties are also important causes of reading disability. Both types of defect are very often associated with speech defects that occur with a certain proportion of reading failures and vice versa.

Ewers (83), in an exploratory study, tries to relate reading disabilities to auditory defects by presenting the hypothesis that in addition to defects of the peripheral nervous system, there also may exist in individuals of normal intelligence and apparent normal hearing some defect of the central nervous system which prevents or impedes learning to read. A great number of auditory tests consisting of 25 group tests administered by Karlin, two reading tests, and the Western Electric 4A Audiometer test were applied on 140 students of the Whiting High School of Whiting, Indiana. The statistical analysis and psychological interpretation of the data showed that, in addi-

XUM

tion to peripheral abilities, it seems logical to assume the involvement of central processes, especially in the ability to react to stimuli of very short duration and the ability to put together two sounds not occurring simultaneously, which means grasping the relation between the two by a closure factor. These abilities may have something to do with the problems of reaction time and attention.

Smith (84) makes an attempt to determine to what degree laterality tests differentiate between retarded readers and reading achievers. The 100 subjects were 9- to 11-year-old boys who ranged in IQ from 90 to 131 for retarded readers and 90 to 125 for reading achievers. The reading quotients of the retarded readers ranged from 69 to 88 and those of the reading achievers from 100 to 120. Handedness of parents, change of handedness, hand, foot, eye, and ear preference evidently have no relation to reading disabilities. The only significant differences between the two groups revealed by this study concern reversals in reading lower case letters and performance on the Van Riper Test of "Central" Dominance at the critical angle of 360 degrees. These findings must be carefully analyzed in new experiments for a better understanding of their psychoneurological implications.

Birch (85) reports very interesting experiments on the improvement of reading ability. A survey of reading ability was carried out with three age groups of ages five, nine, and eleven. It was found that approximately 23 per cent of the tested children had reading ages two years or more below their mental ages. In the secondary modern schools, after the abler third of the population had been creamed off, 40 per cent were retarded by this amount. A remedial center was set up to deal with the worst cases, and teachers were stimulated by lectures, discussions, exhibitions, etc. In 1949, just two years after the first survey, a similar investigation was carried out with one age group in order to detect whether there was any improvement. As a matter of fact, it was shown that the average reading age of the children had risen from 8.9 years in 1947 to 10.1 years in 1949 and that the number of children retarded two years or more had fallen from approximately 40 per cent to 16 per cent. The improvement in reading ability could not be ascribed to a general improvement in conditions as shown by a control of 24 secondary modern school children who were found to be very retarded in 1947, but who could not be given any treatment till 1948 and, accordingly, showed no progress in reading ability. This investigation shows the real value of scientific experimentation combined with treatment in the field of education.

Strang (86) discusses the many different patterns of education which lead to reading improvement, and she shows how students spontaneously improve their reading. It is suggested that reading films should give the students a mechanical stimulus toward reading in larger thought units rather than word by word and that discussion of reading and keeping a diary record of ones daily effort to improve reading are valuable as a help. Correction of physical defects and improvements through insight and individualized practice are recommended.

Kearney (87) investigated the vocabulary of first-grade reading material

in a report based on a study of 121 first-grade readers made up of 42 preprimers, 38 primers, and 41 first readers. Seven hundred and twenty different words were used in the 42 preprimers, 1,400 different words in the 38 primers, and 2,526 in the 41 first readers. The running words were also counted according to arbitrary rules. Frequency tables are given for the words used in 121 first-grade reading books arranged in the order of their frequency of use and grouped according to types of material.

The role of fixation pause duration in reading was studied by Tinker (88) who reports experiments carried out in the laboratory. The minimum pause duration for successive fixation of dots and for successive fixation and identification of single letters averaged 172 and 157 msec. In no case do the pauses take less than 90 per cent of the reading time. It is concluded that pause duration by itself does not represent a valid measure of reading proficiency. The perception time resulting from a combination of pause duration with fixation frequency proves to be a fairly valid measure of reading proficiency. Russell (89) summarizes the investigations in teaching of reading and deals especially with the process of learning to read at various age levels. Experiments on vocabulary, word recognition skill, individual differences, and the different aspects of children's reading activities are reported in this important book for the teacher and the reading expert.

Robinson (90) asked 60 freshmen in Yale college about the reasons for seeking remedial help in reading. Forty per cent indicated a sincere desire to improve scholastic efficiency through remedial work. Some of the men had been advised by a counselor or a faculty member to participate in remedial work, and some were handicapped by speech defects or specific spelling disabilities. Experiments showed that the remedial students really improved their reading ability as compared with a control group. Vernon (91) states that vocabulary level is often regarded as an important factor but cannot at present be satisfactorily assessed. The value of a quality scale is discussed on an experimental basis in order to get better estimates of the difficulty of words.

Eppel (92) designed a test of poetry discrimination in order to study the changes of development in this ability, especially between the sexes and between age groups of senior secondary school and university populations. For both sexes, there is a steady and highly significant increase in total score with increasing age. The general educational implications of the findings are discussed.

Burns (93), in view of the fact that the vocabulary burden is an essential part of the program in teaching French, made an attempt to determine the actual achievement in vocabulary of selected groups of pupils during one year's study of French. The greatest increase in both recall and recognition vocabulary took place in the first term. Recall vocabulary showed an increase of nearly one-half in the second and third terms. During the same period, recognition vocabulary increased by 25 per cent. Gentry (94) selected 66 preprimers and examined them with regard to the vocabulary load. The

XUM

total number of these 66 preprimers was found to have a vocabulary load of 600 different words. A common list of 250 words was established by selecting those words which appeared in three or more preprimers. The books were arranged according to percentage of words found in the common word list. The practical use of the findings for the kindergarten and first-grade teachers are discussed.

McNally (95), in a series of experiments, used the May & Hartshorne duplicating technique in studying classroom cheating in arithmetic and spelling. The main experiment included 312 pupils (144 boys and 168 girls). The cheating was found to be greater in spelling than in arithmetic. No sex differences and no significant consistency in response were found. The concept of honesty as a trait specific to the specific situation is valid and agrees with earlier American experiments. Consequently, one cannot from results of one test tell how any one individual will behave in another, even when the test situations are similar. Northrup and associates (96) used a functional item analysis of group performance in general mathematics in order to show its usefulness to teachers and supervisors in different ways as an aid in locating the pupils most in need of remedial help, as a detector of specific areas of weakness, and as a functional basis for improving both the validity and reliability of objective tests. Bromley & Carter (97) predicted the scores in mathematics for a total of 397 students who completed their mathematics courses at the University of Illinois during the academic year 1947 to 1948. They computed the weights by using the regression coefficients for three measures: the mathematics proficiency, the Q score of the American Council on Education, and rank in high school graduating class. The equation for the predicted score of each individual can be found from these results. Sutherland (98) compared the arithmetical performance of pupils in the secondary school with their performance at the time of promotion and found that all children, and especially the more able, are keyed up to a special effort at the time of the transfer examination and that this leads to an overestimate of the child's normal general ability in arithmetic. Peel & Rutter (99) found that, as a group, the battery of entry tests gives better prediction of subjects like English language and literature, French, and mathematics than of the predominantly scientific subjects. They showed by statistical procedures that the best predictions are significantly close to the maximum predictions.

Manor (100) reports some findings in the field of predicting musical achievement by using the pitch-memory and rhythm tests of the revised Seashore test of musical talent and tests of intelligence and persistence. These tests were given to selected fourth-grade classes. The best factor for predicting tonette success was the persistence ranking with a correlation of .60. Success on the tonette produced the second highest correlation with instrumental achievement, an r of .41. Intelligence tests gave no prediction of success in musical achievement.

Furst (101) studied the relationship between tests of intelligence and tests

of critical thinking. Two samples of students were selected, a group of 60 students from the University of Chicago and a group of 63 students from two public high schools of the Chicago system. It was found that the various tests of critical thinking measure characteristics of student behavior within these groups which are not closely related to scholastic aptitude. Tests of knowledge correlated as high with tests of critical thinking as did intelligence tests.

Williams (102) calculated correlations between the marks of students at Sheffield University in each high school certificate subject and their marks in any subject which they studied in their first year at the university. Thirty of the 68 correlations computed were found to be less than three times their probable error. High positive correlations of .79 existed between the Latin School certificate and later university marks and .77 between principal biology and first M.B. biology. The pedagogical conclusions of these findings are discussed.

Lehman (103) presents data on certain background factors of musicians. Intelligence, musical capacity, and personality were measured by the Otis Intelligence Test, the Kwalwasser-Dykema Music Test, and the Minnesota Multiphasic Personality Inventory. Four hundred fifty subjects participated in this study. Half of this number were subjects who had discontinued musical studies, and half were subjects who had continued studies and had music as an occupation. The latter group earned a significantly higher median in the musical test battery, well above 1 per cent level of confidence. The former group showed a significant tendency to more deviations from normal traits on certain scales of the Minnesota Multiphasic Personality Inventory. Lewis & Shephard (104) report experiments with the Mashburn Apparatus and give evidence of the existence of associative interference in psychomotor performance.

LITERATURE CITED

1. Gesell, A., *The Embryology of Behavior* (Harper & Brothers, New York, N. Y., 289 pp., 1945)
2. Meredith, H. V., and Culp, S. S., *Child Development*, **22**, 3–14 (1951)
3. Bayley, N., *Child Development*, **22**, 47–60 (1951)
4. Frank, L. K., *Child Development*, **21**, 21–24 (1950)
5. Krogman, W. M., *Child Development*, **21**, 25–32 (1950)
6. Greulich, W. W., *Child Development*, **21**, 33–44 (1950)
7. Wechsler, D., *Child Development*, **21**, 45–50 (1950)
8. Jones, M. C., and Bayley, N., *J. Educ. Psychol.*, **41**, 129–48 (1950)
9. Riesch, K. P., *J. Exptl. Education*, **18**, 31–55 (1949)
10. Meredith, H. V., and Meredith, E. M., *Child Development*, **21**, 141–47 (1950)
11. Bennett, S. M., *Brit. J. Educ. Psychol.*, **20**, 45–52 (1951)
12. Siegel, I., *Am. Psychologist*, **5**, 291–92 (1950)
13. Glassey, W., and Weeks, E. J., *The Educational Development of Children* (University of London Press, London, England, 247 pp., 1950)
14. Zur Nieden, M., *J. Psychol.*, **31**, 91–95 (1951)
15. Château, J., *J. Psychol.*, **43**, 332–49 (1950)

XUM

16. Himmelweit, H. T., and Petrie, A., *Brit. J. Educ. Psychol.*, **21**, 9–29 (1951)
17. Morris, W. W., and Nicholas, A. L., *J. Clin. Psychol.*, **6**, 309–19 (1950)
18. Roff, M., *J. Psychol.*, **30**, 199–227 (1950)
19. Wall, W. D., and Simson, W. A., *Brit. J. Educ. Psychol.*, **20**, 153–62 (1950)
20. Willgoose, C. E., *J. Educ. Research*, **44**, 138–42 (1950)
21. McGhee, M., *Brit. J. Educ. Psychol.*, **20**, 144–45 (1950)
22. Carter, G. C., *J. Educ. Research*, **44**, 381–85 (1951)
23. Haines, M. S., *J. Psychol.*, **29**, 259–61 (1950)
24. Rautman, A. L., and Brower, E., *J. Psychol.*, **31**, 263–70 (1951)
25. Kates, S. L., *J. Psychol.*, **31**, 233–41 (1951)
26. Weider, A., and Noller, P. A., *J. Clin. Psychol.*, **4**, 319–25 (1950)
27. Malrieu, P., *J. Psychol.*, **43**, 239–44 (1950)
28. Broida, D., Izard, C., and Cruickshank, W., *J. Clin. Psychol.*, **6**, 243–48 (1950)
29. Trankell, A., *Vänsterhänthet hos barn i skolåldern* (Mercators tryckeri, Helsingfors, Finland, 296 pp., 1950)
30. Doll, E. E., and Walker, K. F., *J. Clin. Psychol.*, **7**, 9 (1951)
31. Lowenfeld, M., *J. Psychol.*, **30**, 325–31 (1950)
32. Bayer, L. M., and Snyder, M. M., *Child Development*, **21**, 93–120 (1950)
33. Harriman, M., and Harriman, P. L., *J. Clin. Psychol.*, **6**, 175–77 (1950)
34. Heuyer, C., *J. Psychol.*, **43**, 525–45 (1950)
35. Valentine, C. W., *Psychology and Its Bearing on Education* (Metheun & Co., Ltd., London, England, 674 pp., 1950)
36. Elmgren, J., *School and Psychology* (Statens offentliga utredningar, Stockholm, Sweden, 1951)
37. Schlesser, G. E., *J. Educ. Psychol.*, **41**, 237–42 (1950)
38. Brown, G. L., *J. Educ. Research*, **14**, 151–53 (1950)
39. Harris, C. W., and Schmid, J., Jr., *J. Exptl. Education*, **18**, 175–93 (1950)
40. Mosier, C. I., *Educ. Psychol. Measurement*, **11**, 5–23 (1951)
41. Spearman, C., and Jones, L. W., *Human Ability* (The Macmillan Co., London, England, 198 pp., 1950)
42. Vernon, P. E., *The Structure of Human Ability* (Methuen & Co., Ltd., London, England, 172 pp., 1950)
43. Knehr, C. A., *J. Psychol.*, **29**, 165–71 (1950)
44. Portenier, L. G., *J. Educ. Psychol.*, **41**, 493–99 (1950)
45. Feifel, H., *J. Educ. Psychol.*, **41**, 1–18 (1950)
46. Lennon, R. T., *J. Educ. Psychol.*, **41**, 301–8 (1950)
47. Belman, H. S., and Evans, R. N., *J. Educ. Psychol.*, **42**, 52–58 (1951)
48. Marks, E. S., *J. Educ. Psychol.*, **42**, 85–96 (1951)
49. Berdie, R. F., *J. Educ. Psychol.*, **42**, 114–23 (1951)
50. Brown, J. L., *J. Exptl. Education*, **18**, 139–46 (1950)
51. Barnette, W., Jr., *Educ. Psychol. Measurement*, **11**, 52–66 (1951)
52. Adkins, D. C., *Educ. Psychol. Measurement*, **11**, 29–51 (1951)
53. Travers, R. M. C., *Educ. Psychol. Measurement*, **11**, 128–37 (1951)
54. Burt, D. C., *Brit. J. Educ. Psychol.*, **20**, 1–10 (1950)
55. Anstey, E., Dowse, R. F., and Duguid, M., *Brit. J. Educ. Psychol.*, **20**, 99–103 (1950)
56. Foulds, G. A., and Raven, J. C., *Brit. J. Educ. Psychol.*, **20**, 104–10 (1950)
57. Bond, G. L., and Fay, L. C., *J. Educ. Research*, **43**, 475–79 (1950)
57a. Kenney, J. J., *J. Educ. Research*, **44**, 129–33 (1950)
58. Borg, W. R., *J. Educ. Research*, **44**, 47–53 (1950)

59. Seashore, H. G., Wesman, A., and Doppelt, J., *J. Consulting Psychol.*, **14**, 99–110 (1950)
60. Seashore, H. G., *J. Consulting Psychol.*, **15**, 62–67 (1951)
61. Goldman, I., *J. Gen. Education*, **4**, 234–40 (1950)
62. Jordan, E., *J. Gen. Education*, **4**, 3–11 (1950)
63. Garrett, H. F., *J. Exptl. Education*, **18**, 91–138 (1949)
64. Anderson, K. E., *J. Exptl. Education*, **18**, 163–74 (1950)
65. Furst, E. J., *J. Exptl. Education*, **18**, 215–28 (1950)
66. Furst, E. J., *J. Exptl. Education*, **18**, 343–51 (1950)
67. Humphreys, L. G., *J. Gen. Education*, **5**, 210–16 (1951)
68. Blanchard, B. E., *J. Educ. Research*, **43**, 507–15 (1950)
70. Carter, H. D., *J. Psychol.*, **30**, 333–40 (1950)
71. Alexander, W. M., *Teachers College Record*, **52**, 278–86 (1951)
72. Hopkins, L. T., *Teachers College Record*, **52**, 270–77 (1951)
73. Lorge, I., and Kruglov, L., *J. Educ. Research*, **43**, 467–74 (1950)
74. Patterson, A. V., *Brit. J. Educ. Psychol.*, **20**, 186–91 (1950)
75. Ammons, R. B., and Manahan, N., *J. Educ. Research*, **44**, 14–21 (1950)
76. Pilliner, A. E. G., *Brit. J. Educ. Psychol.*, **20**, 133–36 (1950)
77. Richardson, J., *Brit. J. Educ. Psychol.*, **20**, 200–1 (1950)
78. Fitch, M. L., Drucker, A. J., and Norton, J. A., *J. Educ. Psychol.*, **42**, 1–20 (1951)
79. Moonan, W. J., *J. Exptl. Education*, **18**, 195–207 (1950)
80. Downie, N. M., Pace, C. R., and Troyer, M. E., *Educ. Psychol. Measurement*, **11**, 76–84 (1951)
81. Schultz, D. G., *Educ. Psychol. Measurement*, **11**, 138–50 (1951)
82. Eames, T. H., *J. Educ. Psychol.*, **41**, 51–55 (1950)
83. Ewers, D. W. F., *J. Exptl. Education*, **18**, 239–62 (1950)
84. Smith, L. C., *J. Exptl. Education*, **18**, 321–29 (1950)
85. Birch, L. B., *Brit. J. Educ. Psychol.*, **20**, 73–76 (1950)
86. Strang, R., *Teachers College Record*, **51**, 82–89 (1950)
87. Kearney, C., *J. Educ. Research*, **43**, 481–93 (1950)
88. Tinker, M. A., *J. Educ. Research*, **44**, 471–79 (1951)
89. Russell, D. H., *Children Learn to Read* (Ginn and Company, Boston, Mass., 403 pp., 1949)
90. Robinson, H. A., *J. Educ. Psychol.*, **41**, 83–96 (1950)
91. Vernon, P. E., *Brit. J. Educ. Psychol.*, **20**, 77–82 (1950)
92. Eppel, E., *Brit. J. Educ. Psychol.*, **20**, 111–16 (1950)
93. Burns, D. G., *Brit. J. Educ. Psychol.*, **21**, 36–44 (1951)
94. Gentry, L., *J. Educ. Research*, **43**, 525–32 (1950)
95. McNally, J., *Brit. J. Educ. Psychol.*, **20**, 137–39 (1950)
96. Northrup, G. J., Pingry, R. E., and Winsor, A. L., *J. Exptl. Education*, **18**, 279–85 (1950)
97. Bromley, A., and Carter, G. C., *J. Educ. Research*, **44**, 148–50 (1950)
98. Sutherland, J., *Brit. J. Educ. Psychol.*, **20**, 3–8 (1951)
99. Peel, E. A., and Rutter, D., *Brit. J. Educ. Psychol.*, **21**, 30–35 (1951)
100. Manor, H. C., *J. Educ. Psychol.*, **41**, 31–50 (1950)
101. Furst, E. J., *J. Educ. Research*, **43**, 614–25 (1950)
102. Williams, E. M., *Brit. J. Educ. Psychol.*, **20**, 83–98 (1950)
103. Lehman, C. F., *J. Educ. Research*, **44**, 57–61 (1950)
104. Lewis, D., and Shephard, A. H., *J. Psychol.*, **29**, 35–46 (1950)

XUM

STATISTICAL THEORY AND RESEARCH DESIGN[1]

By Quinn McNemar

Department of Psychology, Stanford University, Stanford, California

The title of this chapter notwithstanding, our aim here is to consider techniques—the evaluation of papers on modern statistical theory is best left to the mathematical statistician. However, the inclusion of the theory of mental tests (and/or psychological scaling) seems appropriate despite the fact that such theory is not always statistical in the probabilistic sense. Unlike the reviews in the preceding volumes, the present one will not contain a section on general works (textbooks, etc.); rather, such works will be referred to at appropriate places in other sections. As in past reviews, details will have to be omitted; the reader can supply these from the original sources.

Psychometrics

By far the most important publication to appear in this field for years is Gulliksen's *Theory of Mental Tests* (12). This volume not only brings together in one place and in systematic fashion the principal literature on the statistical aspects of mental testing, but also contains original contributions by Gulliksen. Although our chief concern here is with his previously unpublished material, it seems desirable to give the reader some idea of the scope of this excellent volume.

Four chapters are devoted to the concept of measurement error, three to the effect of test length on reliability and validity, and four to the effect of group heterogeneity on reliability and validity; next one finds chapters on a statistical criterion for judging whether tests are parallel, on experimental methods for obtaining test reliability, on reliability via item homogeneity, on speed versus power tests, and on methods of test scoring, then, a lengthy chapter on methods of standardizing and equating scores (including derived scores) and a chapter on the problem of weighting and differential prediction, and finally, a 30-page chapter on item analysis.

The first previously unpublished material has to do with the effect of multivariate selection. Formulas have long been available for correcting the correlation between two variables for known selection on one of them. After presenting formulas for use in ascertaining the effect on correlation between Y and Z of explicit selection on X, Gulliksen proceeds to develop (in Chapter 13) the rationale for multivariate selection—the effect on correlation of explicit selection on more than one variable. The interested reader will first brush up on matrix algebra and turn to Gulliksen for detail.

[1] The survey of the literature to which this review pertains was completed in May, 1951.

The second original contribution has to do with a criterion for determining whether a test is primarily a pure speed test or a pure power test. Because of the essential difference between speed and power tests, Gulliksen elaborates on the appropriate methods for determining their reliabilities.

The third new idea is concerned with the rationale for item selection. The end result is essentially a method for selecting items to maximize reliability (a version of selection by internal consistency) and to maximize validity of total score. Let x stand for test score, y for the criterion measure, and g for an item; then an item "reliability index" is defined as $r_{xg}\sigma_g$ and an item "validity index" as $r_{yg}\sigma_g$, where the r's are point biserial coefficients and σ_g is the item standard deviation.

Since no consideration was given in the previous *Annual Reviews* to a paper by Mollenkopf (23), and since a simplified version of the paper is given by Gulliksen (12, pp. 115–24), it would seem appropriate for the present reviewer to make a few remarks thereon. What Mollenkopf (and presumably Gulliksen under whom the research was done) purports to do is to investigate analytically the question whether, in general, the magnitude of measurement error is independent of test score. (It is known, for example, that independence does not hold for tests utilizing ratio IQ's—the higher the IQ the greater the error of measurement.) Mollenkopf (and Gulliksen) arrives at the conclusion that the error of measurement will be related to test score if, for the distribution of test scores, the skewness and kurtosis depart from that for the normal curve. To the reviewer, this simply does not make sense. Perhaps the outcome is due to the assumption (12, p. 120) that the correlation between errors of measurement and the cube of the deviation scores is zero, that is, it is assumed that error and score are uncorrelated. Since the analysis is not one in which something is assumed to be zero for the sake of proving that it cannot be zero, it seems likely that this assumption is unwarranted; if we accept their conclusion, the assumption certainly becomes untenable as soon as we have the specified condition for a nonzero relationship. The reader who happens to have perused (and been impressed by) Mollenkopf's empirical demonstrations should be apprised of the fact that no significance tests were applied. A correlation between errors of measurement and test scores implies heteroscedasticity for the form versus form reliability scatter diagram, hence the hypothesis of a relationship is readily testable by use of Bartlett's test for homogeneity of variance [see (9, p. 196)].

Another out of the ordinary volume appeared during the year, *Measurement and Prediction* by Stouffer, Guttman, Suchman, Lazarsfeld, Star & Clausen (30). This, the fourth volume in the "American Soldier" series, contains an extensive discussion of scale analysis by Guttman, with two chapters by Suchman on the Scalogram Board technique for scaling items. Those who have been struggling over previous expositions of this type of scaling will find enlightenment here. In addition, Guttman contributes a chapter on "The Principal Components of Scale Analysis." The reviewer

XUM

is unable to understand either the English half or the mathematical half of this chapter. It would seem that the first and most principal component corresponds to a metric component and will be perfectly related to the rank ordering of individuals. The second component is identified with intensity (of attitude), while the third is tentatively called "closure." The confusing thing here is that these several components (n in number for n "types of items") underlie items that are scalable, that is, that are functions of one variable (unidimensional), and yet the components are supposedly of the same mathematical character as Hotelling's principal components (or Thurstone's principal axes). Our only further comment is that we still have with us the problem of making psychological sense out of an idealized mathematical model.

This volume also contains the first comprehensive discussion of "latent structure theory" by Lazarsfeld. This purports to be a generalization of the Spearman-Thurstone factor analysis to include qualitative material, such as responses to opinion questions. It is postulated that underlying the manifest (or given) responses, there exists a set of latent classes which will account for the relationships between the responses to items. The simplest case, the latent dichotomy model, involves two latent classes—individuals either possess or do not possess the latent character. The general theory postulates more than two latent classes—a latent structure. Although this structure does not imply an ordering of the classes, it is interesting to note that provision is made for the case involving order, i.e., a continuum, and that the theory as yet has little to offer in the case of more than one underlying continuum—the generalization of factor analysis is not complete.

Latent structure analysis and factor analysis share a common deficiency: for neither method do we have statistical criteria for judging the fit of data to the postulated mathematical model. Lazarsfeld has little to say about the psychological meaning of the variable (or continuum) underlying a set of latent classes. It is suggested (30, p. 363) that such a continuum might be "soldier morale, anti-Semitism or intelligence." If so, it follows that a latent dichotomy or trichotomy means two or three latent classes with continuity within the classes and no sharp demarcation between the classes. It would be premature to attempt a further evaluation of a theory and technique which is admittedly still being formulated. At least one qualified mathematical statistician (10), though aware of as yet unsurmounted difficulties, makes no criticism of the fundamental ideas.

Another approach to the scale problem may be found in a paper by Coombs (7), who proposes a method of scaling without a unit of measurement. This "unfolding technique" consists essentially in converting rank orders into scale distances such that the distances with respect to each other are merely characterized as being greater or less. This article is a condensation of a more elaborate forthcoming presentation in which the unfolding technique will be compared and contrasted with Guttman scaling and with

the method of paired comparisons. For Thurstone's Case V of the last named method, Mosteller (26) shows that Thurstone's restriction of zero intercorrelations among the sets of stimuli can be replaced with the less restrictive assumption that the intercorrelations are approximately equal.

The old problem of correcting for guessing in multiple-choice tests has been reopened by Hamilton (13) who attacks the validity of the standard procedure in use for a quarter of a century. This procedure, it will be recalled by most readers, involves the formula $S = R - W/(k-1)$ for a k-choice test, with R as unadjusted score and W as number of incorrect answers. Hamilton argues that the adjustment should be made via a regression equation which, according to his derivation, is of the form

$$S' = \frac{(k\overline{R} - n)R}{(k-1)\overline{R}}$$

in which $\overline{R}$ is the mean of unadjusted scores based on a k-choice test of n items. Lyerly (19) questions this result on the very reasonable ground that one should be able to adjust a score for guessing without any reference whatsoever to a group or to a group mean. He then proceeds to show that the classical formula is a maximum-likelihood (and also unbiased) estimate of an individual's "true score." This implies an error in Hamilton's reasoning or in his derivation or an error on Lyerly's part. Both cannot be right. The reviewer is skeptical of one of Hamilton's assumptions, namely, that the test scores are binomial in distribution; it is simply contrary to the facts to assume that the covariances among the test items are zero.

Factor Analysis

The papers appearing during the year have been concerned mainly with the statistical (probabilistic) aspects of factor analysis. In a mathematically difficult paper by Reiersol (27), conditions are set forth for the "identifiability" of the parameters in Thurstone's simple structure method, i.e., the requirements for uniqueness of the elements in the rotated factor matrix (and consequently, the communalities). This also includes the question of the number of columns in the matrix, i.e., the number of identifiable factors. His paper is not concerned with the sampling problem, but rather with the number of variables, considering their intercorrelations in a hypothetical population, required for identifying a specified number of factors.

Bartlett (2) has developed a method for testing the significance of residuals (as a group) when the analysis is made by the principal components method, but this is small comfort to the large majority of factor analysts who prefer the Thurstone centroid method (and rotations). The earlier proposed χ^2 test by Lawley (17) has been subjected to an empirical check by Henrysson (15) who finds that the test does work for a case of nine variables with one general factor extracted by Lawley's maximum-likelihood method. In discussing Henrysson's paper, Burt (3) proposes treating the problem via

partial correlation (computing the residuals as partials) since something is known about the sampling behavior of partial correlation coefficients. Burt apparently was not aware that the same proposal, with elaborations, had been published in 1942 in an American journal (20).

Prediction or Classification

As is well known, the discriminant function technique is one in which two or more variables are so weighted as to give the maximum discrimination between two groups. This permits the classification of individuals into the two groups with a minimum of misclassifications and, hence, is a device for predicting to which group an individual belongs. It is also well known that the technique is closely allied with multiple correlation. Attempts are currently being made to develop a multiple discriminant function, one that will permit optimum classification into three or more groups. One such attempt is that of Anderson (1) who develops a rationale and presents a worked out example involving four variables and three groups. Another approach is that of Lubin (18) who proceeds from an analysis of variance and correlation ratio idea to a method for maximizing differences between j groups on t tests by assigning weights to the tests. The solution, which involves extraction of the latent roots of a matrix, may or may not be sufficiently practical to solve the problems of vocational guidance and of psychiatric diagnosis by way of a battery of tests. A symposium at the 1950 American Psychological Association Meetings indicates that a workable multiple discriminant function is forthcoming.

Another aspect of the prediction problem, differential prediction, has received some attention. Given two graduated criteria with two multiple regression equations (involving the same independent variables), Mollenkopf (24) has shown (*a*) that the regression weight for predicting differences in criterion scores equals the difference in the weights for separate prediction of the two criteria, (*b*) that a predicted difference between criterion scores equals the difference between predicted criterion scores, and (*c*) that the multiple correlation for predicting differences in criterion scores is a function of the two multiple r's for the two criteria, the degree of correlation between criterion measures, and the correlation between the predicted criterion measures.

Differences of another sort have been discussed by Wittenborn (33). He questions the practice (in clinical psychology) of using $D = X - Y$ in diagnostic (or predictive) work. He points out the rather obvious fact that since all the variance in D is completely determined by X and Y, one cannot expect D to lead to better (even as good) prediction as that obtainable by the optimum weights of multiple regression or of the discriminant function. If optimum prediction is possible only by way of a difference score, the multiple regression equation will involve one negative regression coefficient.

A review and general discussion of statistical techniques and diagnostics

by Mensh (22) appeared during the year. He stresses the fact of nonnormality of many variables used in clinical research, then proceeds to discuss statistical techniques most of which assume normality. The present reviewer is of the opinion that one of two things must be done to rescue clinical research: either quantify the variables in such a way as to obtain normal distributions or develop statistical methods for handling the rather peculiar (as to distribution) "measurements" that are available.

Attributes

A test of the significance of the differences between correlated proportions (or percentages) for more than two groups has been developed by Cochran (4). This is an extension or generalization of the χ^2 test recently (21) given for comparing two correlated proportions (two groups matched by pairing or two proportions based on the same group). The generalization provides a means of testing the over-all differences between proportions (*a*) based on k matched groups, (*b*) on k items for the same group, or (*c*) obtained on one item from the same group under k different conditions.

The computations for the test are relatively easy to perform. Suppose we have C items which are answered yes or no by N persons and that the data have been arranged in C columns and N rows, a row for each person. Let u_i stand for possible number of yeses by a person; u_i can take on values of 0, 1, 2, $\cdots$, C. Let f_i stand for the frequency of u_i yeses; f_2, e.g., would be the number giving two yeses. Let T_j be the number of yeses in the jth column, and let $\overline{T}$ be the mean of the T_j's. The f_i constitute a frequency distribution for the u_i. Calculate $\Sigma f_i u_i$ and $\Sigma f_i u_i^2$. Then the quantity

$$Q = \frac{C(C-1)\Sigma(T_j - \overline{T})^2}{C\Sigma f_i u_i - \Sigma f_i u_i^2}$$

has the χ^2 distribution with $C-1$ degrees of freedom, providing N is not small.

Though not properly subsumed under the heading of this section, a note by Wilkinson (31) will be given consideration at this place. Suppose one has adopted $P = .05$ as a working level of significance, and suppose further that m sets of data have been analyzed, i.e., we have carried out m (independent) tests of significance. Wilkinson provides tables for determining the probability of n significant P's arising from the m comparisons. For example, if four sets of data are available, thus permitting four tests of significance, the probability of obtaining one CR (or t or F) significant at the .05 level is .18. Two tables are presented, one for the .05 level and one for the .01 level, for varying m's and n's. The problem here is not to be confused with that discussed in Cochran & Cox (6, p. 68) concerning probabilities when the largest of several differences is selected for testing.

Heterogeneity of Variance

The question whether the differences between several variances are significant arises either because of an intrinsic interest in the variabilities of groups

or because of the homogeneity of variance assumption in the analysis of variance technique. The latter is amply stressed in the recent book by Edwards (9). The Bartlett test for homogeneity of variance is most frequently used when three or more variances are to be compared, and the two-tailed F test is appropriate when we have only two variances. Although the computations for the Bartlett test are not particularly laborious, Hartley (14) has proposed a shorter test. He derives the sampling distribution and provides a table for the .05 level of significance for F_{max} which is defined as the ratio of the highest to the lowest of the several available estimates. It is assumed that the degrees of freedom for the two estimates are the same or not too unequal.

Green (11) has developed a maximum-likelihood ratio method for testing the hypothesis that standard errors of measurement are equal from group to group. Application of the method requires the use of Wilks' criterion for equality of means and variances of the parallel forms (or parallel halves) of the psychological test being used (32). He fails to point out that the Wilks criterion assumes normality of score distribution, so presumably one should first test for normality. Close scrutiny of the Green test reveals that, when the several group N's are large, it is practically identical with the Bartlett test for homogeneity of variance and that it is not as widely applicable as the latter which is adequate for situations involving N's smaller than 50, the lower limit for Green's test.

Analysis of Variance

The most ambitious attempt to date to write for psychologists a text under the label of "experimental design" is that of Edwards (9). The book leans heavily on the analysis of variance technique; all but the simplest designs involve the F test. It is not within the scope of this review to evaluate Edwards' book, but in passing we will mention one of its features and inject a word of caution concerning one of the chapters.

Edwards' approach is much like that of Snedecor (29) in that the ideas are developed mainly through worked out examples, and like Snedecor, he generally fails to present computational formulas. This is a pedagogical matter which does not detract from the soundness of the book. The thing that disturbs the present reviewer is that 30 pages are devoted to "selling "the latin square design to psychologists without giving an inkling of a fundamental assumption underlying its use. The mathematical model for analysis of variance based on this design definitely assumes that all interactions are zero (25, p. 340). Since practically all the proposed uses of the latin square in psychological research involve the assignment of individuals to rows (or columns or cells) and since interactions involving individuals are usually not zero, it follows that a basic assumption is being violated. The reviewer, though formal mathematical proof is lacking, is reasonably sure that failure to meet the assumption leads to too many significant F's.

Cochran & Cox's *Experimental Designs* (6), a most comprehensive and

authoritative book on the subject, was published during the year. After presenting a concise summary of the methods of statistical analysis, they proceed to a delineation of the numerous designs that have been found useful for research in agriculture and biology. Whether or not many of these designs can be translated to psychological problems remains to be seen; it is to be hoped that the little matter of assumptions is not ignored when translations are made.

Another book of significance to appear during the year is the text on mathematical statistics by Mood (25). We mention it here because of his excellent discussion of the components of variance model in the analysis of variance, the model which is most apt to be involved in so-called factorial designs in psychology.

In an article entitled "Testing a Linear Relation among Variances," Cochran (5) gives a method for testing the significance of a main effect for the components of variance model involving nonvanishing interactions. By writing out the expected values for the several variance estimates in a three-way classification problem, he points out that frequently one may set up a function as the difference between combinations of variance estimates in such a way that the difference will reflect a main effect, hence, permit a valid test of the main effect. Rules are given for determining the degrees of freedom for the obtained F ratio.

The present status of the relatively new variance component analysis has been discussed by Crump (8). Suppose we have a two-way classification, or two-factor, setup with n cases per cell and that factor B represents learning methods and factor A involves a random sampling of school classes, a in number. Since the computed variance estimate for schools will involve two components, one due to school differences and one due to the school by method interaction, in order to estimate the school variance, the mean square for interaction is subtracted from that for schools and the difference divided by $(n)(a)$. Formulas are provided for the sampling variance of such an estimated variance.

Frequently, an experimenter using an analysis of variance design or a simpler design raises the question whether more is to be gained in the over-all precision of his experimental results by increasing the number of measurements per case than by increasing the number of cases. Jarrett & Henry (16) have made explicit the relative advantages of these two ways of increasing the accuracy of a mean. As one might expect, the relative effectiveness of the two ways is a function of the reliability coefficient. The important generalization is that even when the reliability is very low it is better to take more cases. Perhaps the next needed step is to set this up as a cost function to determine the most economical plan with due regard to the relative costs of capturing more cases versus increasing the number of measurements per person.

In an expository article entitled "A Synthesis of Experimental Designs in

Program Research," Royce (28) has developed the thesis that the logical steps in experimentation are (*a*) make a multiple factor analysis (Spearman-Thurstone type) to determine the factorial content of the variable to be studied and construct tests for the factors which are contributing to the variable, (*b*) set up a super factorial design (analysis of variance type) to test dependence of the factors on as many variables as we wish information about, then (*c*) return to the single variable experiment to work out the functional relationships of the dependent variable, or rather its factor components, with the several variables utilized under (*b*). The merit that one sees in this proposed approach will depend upon (*a*) one's faith in the outcome of multiple factor analysis, (*b*) one's ability to make sense out of higher order interactions and also out of the main effects when interactions are present, and (*c*) the meaning of the functional relationship between a dependent variable and a single variable when dealing with a multivariate system. Royce demands that we "attack multiple variable domains with multiple variable techniques," yet he fails to point out how his third step (single variable experiment) is to be used to construct the multivariate relationships. Surely, a functional relationship in a multiple variate domain must some way or other bring together the many variables so as to permit the writing of an equation of the form $y=f(x_1, x_2, x_3, \cdots, x_n)$. It is difficult to see how the form of (and constants for) such a multivariate functional relationship can be ascertained by resort to varying one x at a time. There are, of course, many psychologists who are skeptical about individual difference variables (arbitrarily defined, or those resulting from factor analysis) ever being involved in functional relationships.

LITERATURE CITED

1. Anderson, T. W., *Psychometrika*, **16**, 31–50 (1951)
2. Bartlett, M. S., *Brit. J. Psychol., Statist. Sect.*, **3**, 77–85 (1950)
3. Burt, C., *Brit. J. Psychol., Statist. Sect.*, **3**, 161–65 (1950)
4. Cochran, W. G., *Biometrika*, **37**, 256–66 (1950)
5. Cochran, W. G., *Biometrics*, **7**, 17–32 (1951)
6. Cochran, W. G., and Cox, G. M., *Experimental Designs* (John Wiley & Sons, Inc., New York, 454 pp., 1950)
7. Coombs, C. H., *Psychol. Rev.*, **57**, 145–58 (1950)
8. Crump, S. L., *Biometrics*, **7**, 1–16 (1951)
9. Edwards, A. L., *Experimental Design in Psychological Research* (Rinehart & Co., New York, 446 pp., 1950)
10. Girshick, M. A., and Lerner, D., *Public Opinion Quart.*, **14**, 710–28 (1950)
11. Green, B. F., *Psychometrika*, **15**, 251–57 (1950)
12. Gulliksen, H., *Theory of Mental Tests* (John Wiley & Sons, Inc., New York, 486 pp., 1950)
13. Hamilton, C. H., *Psychometrika*, **15**, 151–68 (1950)
14. Hartley, H. O., *Biometrika*, **37**, 308–12 (1950)
15. Henrysson, S., *Brit. J. Psychol., Statist. Sect.*, **3**, 159–61 (1950)

16. Jarrett, R. F., and Henry, F. M., *J. Psychol.*, **31,** 175–80 (1951)
17. Lawley, D. N., *Proc. Roy. Soc. Edinburgh*, **61,** 176–85 (1941)
18. Lubin, A., *Brit. J. Psychol., Statist. Sect.*, **3,** 90–103 (1950)
19. Lyerly, S. B., *Psychometrika*, **16,** 21–30 (1951)
20. McNemar, Q., *Psychometrika*, **7,** 9–18 (1942)
21. McNemar, Q., *Psychometrika*, **12,** 153–57 (1947)
22. Mensh, I. N., *Psychol. Bull.*, **47,** 475–92 (1950)
23. Mollenkopf, W. G., *Psychometrika*, **14,** 189–229 (1949)
24. Mollenkopf, W. G., *Psychometrika*, **15,** 409–17 (1950)
25. Mood, A. M., *Introduction to the Theory of Statistics* (McGraw-Hill Book Co., Inc., New York, 433 pp., 1950)
26. Mosteller, F., *Psychometrika*, **16,** 3–9 (1951)
27. Reiersol, O., *Psychometrika*, **15,** 121–49 (1950)
28. Royce, J. R., *J. Gen. Psychol.*, **43,** 295–303 (1950)
29. Snedecor, G. W., *Statistical Methods* (The Iowa State College Press, Ames, Iowa, 485 pp., 1946)
30. Stouffer, S. A., Guttman, L., Suchman, E. A., Lazarsfeld, P. F., Star, S. A., and Clausen, J. A., *Measurement and Prediction* (Princeton Univ. Press, Princeton, N. J., 756 pp., 1950)
31. Wilkinson, B., *Psychol. Bull.*, **48,** 156–58 (1951)
32. Wilks, S. S., *Ann. Math. Statistics*, **17,** 257–81 (1946)
33. Wittenborn, J. R., *J. Clin. Psychol.*, **7,** 108–11 (1951)

XUM

MOTIVATION[1]

By O. H. Mowrer

Department of Psychology, University of Illinois, Urbana, Illinois

Considering its prominence in present-day psychology, the concept of motivation has had a surprisingly meager history. In the latter part of the nineteenth century, the common explanatory categories in psychology were: "instinct," "reason," "repetition," "volution," and "hedonism." Motivation, in the sense of drive, had almost no place at all in scientific thinking. Men of letters sometimes referred to "motives," but it was only with the work of Freud (10) and Thorndike (45), in the nineties, that this concept began to have scientific currency.

Hedonism is in some ways the most direct ancestor of modern motivation theory, but there is an important distinction. Hedonism, as propounded by Bain (3) and others, held that we are "propelled by pleasure and repelled by pain." And it is possible to find traces of this thinking in the works of both Freud (11) and Thorndike (49). But the notable difference is that, with the two latter investigators, pleasure ceased to be a drive in and of itself and was conceived as the experience which occurs when a drive, or motive, terminates, i.e., is satisfied, relieved, or fulfilled. While there are thus identifiable continuities with hedonism and other nineteenth-century explanatory concepts, it is sufficiently evident that motivation theory of today has real novelty and independence.

Motivation Theory: Historical Development

Full appreciation of contemporary theory and research in the field of motivation requires that we review briefly the advance of knowledge in this area during the past half century.[2] Thorndike (46) and the other early investigators of animal behavior under laboratory conditions worked almost exclusively with what are now referred to as primary drives, i.e., biologically given drives such as hunger and thirst; and the drives which Freud (11) emphasized, notably sex and aggression, were likewise regarded as having an "instinctual" basis.

From this background emerged the concept of complacency (equilibrium, adjustment, consummation, homeostasis). The general formula for behavior was assumed to be: drive, variable ("random") behavior, a final correct response, reduction of the drive, and "fixation," or "stamping-in" of that response. Motivation was thus seen to be intimately associated with learning: its presence produced varied performance, and its reduction

[1] The survey of literature to which this review pertains was completed in June, 1951.

[2] Most of the historical events and issues referred to in this section have been discussed in greater detail by Mowrer (24) and Miller (20).

selectively reinforced the correct, effect response.[3] Here, then, was the basic paradigm for biological self-regulation.

But there was a complication. It was apparent, from everyday as well as laboratory observations, that drives can also "inhibit," or "stamp-out," behavior. How was this possible? Still under the sway of the older pleasure-pain psychology, Troland (51) tried to justify a division of drives into two qualitatively different categories: drives, or stimuli, that were "beneceptive" (pleasant, beneficient) and drives that were "nociceptive" (unpleasant, noxious). This effort led only to confusion and sterility. Thorndike's position, though it called for later revision, was basically sounder. It was, in essence, that responses are strengthened by drive reduction ("reward") and weakened by drive increment ("punishment"). Thus, as Muenzinger & Fletcher (30) have more explicitly pointed out, all drives may be thought of as similar: all are "painful," all are capable of producing behavior if they precede or accompany it, all reinforce behavior when they undergo diminution, and all inhibit behavior if they follow it.

From the standpoint of this formulation, many questions—such as, "Which is the more effective, learning by reward or by punishment?"—become quite meaningless. It was clear that reward and punishment involve different relationships, different mechanisms and functions, and are complementary rather than comparable. But the fact of punishment brought investigators in this field face to face with a difficult problem. Thorndike's Law of Effect—that responses are strengthened by reward and weakened by punishment—was often criticized as teleological; it was said to hold that responses were influenced by their consequences. As far as reward learning was concerned, this objection was easily answered: it is not, of course, a specific, episodic response that is influenced by the effects it produces; the response may be completed well before the consequences occur and hence is incapable of being influenced thereby. Rather, it is the organism-making-the-response that (being still existent) is influenced by ensuing drive reduction and is thus modified so as to be more likely to make the same response when the same drive recurs in the future.

But the imputation of teleology was not so easily handled when a response is followed, not by reduction in an already present drive, but by an increase in that drive or by appearance, *de novo*, of another drive. How was it possible, without assuming teleology, to account for the fact that an organism may be deterred from making a given response out of sheer prospect of punishment? How, in other words, could something which has not yet happened inhibit or otherwise control behavior? Thorndike's answer was simply this, that if past occurrences of a stimulus-response sequence, or habit, have been followed a number of times by punishment, the "connection" between the stimulus and response will have been sufficiently weakened that on subsequent occasions the stimulus (drive) will have little

[3] Koch (14) has acknowledged the intimacy of motivation and learning phenomena by the phrase, "Learning-motivation interrelations."

or no tendency to evoke the response in question. Thus, the response would fail to occur, not because of a forward-looking anticipation of punishment, but, quite simply, because the formerly extant stimulus-response "connection," or "association," is now virtually obliterated, nonexistent, "stamped out."

But in the light of clinical, as well as everyday, observations, this type of interpretation was not very convincing. It left no place for the only too real experience of conflict which is likely to arise when a motivated response has been followed, on one or more occasions, by punishment. Habits, we find, do not quietly fade away under the impact of punishment. There is, in other words, much more to response inhibition through punishment than Thorndike's simple formulations implied.

At least a part of the answer was provided by a different type of research. As a result of the work of Bechterev (4) and others, it was well established by the mid-thirties that "punishment," i.e., a sudden, relatively intense increase in drive, could be used to instigate behavior as well as to inhibit it. Thus, if a signal, such as a buzzer, regularly precedes an electric shock to the forepaw of a dog, the dog will soon lift the paw in response to the buzzer alone, in advance of the shock. This finding was hardly a surprise to anyone who had ever driven a team of horses, for here it is obvious that a touch of the whip after an unheeded spoken command causes that command to take on motivational value. But this was an "effect" which was not recognized in Thorndike's Law of Effect. Actually, it should make no difference whether the stimulation which precedes a sudden drive increment is in the form of an external signal—like a buzzer or a "get-up"—or is an internal signal, such as provided by the proprioceptive and other stimuli accompanying a particular response or act: the learning involved in both cases should, in principle, be the same. It was therefore far from parsimonious, not to say an outright contradiction, to assume that there was "positive" (connection-strengthening) learning whenever the signal was external (i.e., in "avoidance conditioning") and "negative" (connection-weakening) learning whenever the signal was internal (as in "punishment").

The difficulty was resolved by simply assuming that any stimulus which precedes an intense drive increment acquires the capacity to arouse *fear* and that this secondary, or acquired, drive will then motivate the organism, just as a primary drive will, to engage in whatever type of behavior will most satisfactorily reduce or eliminate it. Under ordinary circumstances, the best way to eliminate fear is to get rid of the danger signal producing it. And if the signal is an external one, then flight is likely to be effective, whereas if the signal is internal (proprioceptive, action-produced), then immobility is the best way to be "safe." By thus positing fear as an intervening variable in both instances, the paradox is eliminated—and so also is one half of the Law of Effect—at least as Thorndike formulated it.[4]

[4] If, following punishment for an action, an organism is statistically less likely to engage in that action, it is, we now assume, not because the neural traces, or "con-

Within the past decade a number of investigations have validated this type of thinking. It has been shown, for example, that if a response "turns off" a danger signal immediately, it is learned more readily than if the termination of the danger signal is delayed. It has been shown that, by using an appropriate experimental procedure, one can train an animal to make a response to an external warning signal that is radically different from the response which would occur if the drive increment (e.g., shock), of which the signal is premonitory, actually occurs. And it has been shown that escape from a situation in which electric shock or other intense discomfort has been experienced is powerfully rewarding and will reinforce (through reduction of the attendant fear) whatever type of behavior is instrumental in effecting the escape (20, 24).

All in all, the evidence is now clearly in favor of positing the secondary drive of fear as an intervening variable, regardless of whether the "punishment" occurs because a response is made or because a response, following a "command" (signal), is not made. Behaviorally the results are very different —inhibition in the one case and instigation in the other—but there is evidently no difference in principle. The alternative is to assume, as did Thorndike, that when a sudden and relatively intense drive increment follows an act, the effect is to "weaken the connection" between that act and the drive that produced it, and to assume—as Thorndike, Hull, and others have—that when a drive increment follows an external stimulus, there is a form of "associative shifting," whereby the act which the drive increment produces gets switched over and attached to the formerly neutral stimulus. That there should be two such very different mechanisms operating in circumstances which, in essence, are so very similar seems highly improbable. Both hypotheses seem too self-consciously "objective." By simply assuming that the main, or at least the most immediate, result of having a stimulus (of whatever kind) followed by drive increment is the acquisition of the secondary motive of fear, the differential effects produced when the stimulus is external and when it is internal can be readily deduced, as can both the objective and subjective phenomena associated with conflict (8, 19, 21).

---

nections," which formerly mediated the action are obliterated but rather because, through the agency of fear, strongly competing response tendencies have been established. (This conflict theory, as opposed to a stamping-out theory of response inhibition, applies most specifically to responses mediated by the central nervous system and the skeletal musculature, i.e., to "behavior." At present, it is not entirely clear what the mechanism is whereby "autonomic" responses are modified in the negative direction. A conflict theory may be likewise applicable here to the extent that the sympathetic and parasympathetic divisions of the autonomic nervous system are antagonistic and thus capable of checking and balancing each other. But there are unsolved problems: What, for example, is the mechanism involved when a fear reaction is "extinguished" simply through many non-reinforced elicitations? Or, what occurs if a fear reaction—which is a product of punishment—is then itself punished for occurring?)

XUM

This, then, is a brief review of the evidence and arguments by which the concept of secondary motivation has been added to that of primary, biologically given motivation. But in effecting this systematization of the facts, another inference has been strongly suggested, namely that there are two basic forms of learning: *solution learning*, according to which a response that reduces a drive becomes more firmly attached to that drive, be it primary or secondary; and *sign learning*, on the basis of which the secondary drives become attached to signals, as a result of the contiguity of these signals with primary drives. If a signal is paired with, i.e., betokens, drive increment, the secondary drive which is acquired is fear. If a signal is paired with drive decrement (as in the Pavlovian type of salivary conditioning), the secondary drive acquired is appetite. If a signal is paired with a situation involving frustration, the drive, or emotion, which is learned is anger. And if a signal is paired with a situation which produces nausea, the emotion of disgust is developed. All of these secondary motives, or emotional drives, are learned, we assume, on the basis of pure contiguity, or temporal association, whereas instrumental acts, be they prompted by primary or secondary drives, are learned, presumably, on the basis of a different principle, namely that of drive reduction, or reward. On the basis of solution learning, living organisms acquire instrumental acts, or *means;* on the basis of sign learning, they acquire emotions, secondary drives, *meanings*. Although the issue is still an open one, there is growing evidence for believing that this distinction is a basic and valid one (24).

Thus far the discussion has been largely on terms of "stimulus-response psychology." This section would be incomplete if reference were not also made to the problem of motivation as viewed from the standpoint of "ego psychology." Here the reasoning has been more vague and the research less pointed, but from clinical and other sources have come observations and conjectures of some importance. At the level of ego psychology, there may be said to be only one master motive: anxiety. This is the discomfort, or "pain," which is experienced when the individual's "self-system" (44) is threatened with impairment or dissolution. In lesser intensity, it is the drive which is activated when there is said to be "ego involvement" (37) or "interest" (2). "Self-actualization" (12) is achieved when there is a harmonious relationship between all of the person's segmental drives and aspirations; when these are in conflict, there is a weakening of the self-system, with resulting strain upon that system, which is perceived, in varying degrees of intensity, as psychological tension.

Put differently and more concisely, the basic notion here is that the thing that makes a self or self-system is the integration which has been achieved between the constituent parts of the personality, the innumerable conflict resolutions that have accrued as the product of the individual's (and his forebearers') past experience and learning. And when this integrity, unity, wholeness, or harmony of the person-as-a-whole is threatened, the resulting experience is anxiety.

Anxiety, it is assumed, is different from all the primary drives and all

derived drives in that they are segmental and have a kind of impersonality about them. Anxiety, on the other hand, is the "fever of the soul" and is experienced as striking at and involving the very core of the personality. A well-integrated person can, if properly motivated, endure great hunger, thirst, sex drive, fear, and even physical pain; but anxiety is something that is never taken lightly or willingly endured, for it signifies that the very system according to which other drives are subordinated, ordered, and gratified is endangered and that personal disorganization and psychic chaos may be imminent.

Present Status of the "Punishment" Problem

Despite the simplicity of the logical analysis and the supporting experimental evidence which have been reviewed in the preceding section, there continues to be much confusion and controversy regarding the role of "punishment" in motivation and learning theory. In this section, three of the foci of difficulty will be briefly considered.

Beginning in 1934 and extending over a period of several years, Muenzinger and students (28 to 31) reported a series of experiments with white rats in which it appeared that punishment tended to fixate responses instead of inhibiting them. For a long while, this paradoxical finding went unexplained, until Wischner (55) recently called attention to the fact that the Muenzinger experiments had required that all animals, whenever they made an error (in the modified Yerkes-Watson discrimination apparatus employed in these studies), retrace to the choice point and make the correct turn before the trial ended, i.e., before they could get out of the apparatus. When Wischner repeated Muenzinger's work employing a noncorrection procedure, the paradoxical results did not appear; instead it was found that the animals behaved as was to be expected on the basis of traditional assumptions. Wischner found that animals which were rewarded (with food) for the correct choice and shocked for the incorrect choice learned best; that animals that were rewarded for the correct choice and not shocked for either the correct or incorrect choice learned next best; and that animals that were both rewarded and shocked for correct choices learned least well.

The author (24), following the lead thus provided by Wischner's experiment, has shown how, by a proper analysis of the role of secondary motivation—especially fear, which Muenzinger and co-workers largely ignored—one can readily account for the Muenzinger findings without at all denying the specificity of the effects of punishment. The argument will not be repeated here, but it now seems likely that in the Muenzinger experiments we have a neat example of what Hull once referred to, in another context, as "misplaced scientific ingenuity." By devising an experimental procedure which was indeed ingenious but which involved factors which were not completely recognized, the investigators produced results which, without proper analysis, seemed to provide "proof" of a very remarkable proposition: namely that, contrary to traditional belief, living organisms learn to make punished responses rather than to inhibit them.

XUM

One is here reminded of Watson's (52) "disproof" of the Law of Effect (or at least that part of it having to do with reward) by an experiment which seemed to show that learning does not vary with length of delay between a correct response and primary reward (hunger reduction). Later research (35) showed that Watson, in his preoccupation with "objectivity," had simply overlooked the phenomenon of secondary motivation and reward. It appears that the Muenzinger findings are likewise paradoxical only if one neglects the role of fear and the reinforcing effect of its reduction.

At about the same time, or a little later, Thorndike (47, 48) reported a series of studies which seemed to support those of Muenzinger and his students. The basic method was this. Human subjects were presented, repetitively, with a long series of English words and were asked to respond to each of these words with a number from one to ten (or in some equivalent manner). If the number selected in response to a particular word was "correct" (by arbitrary prearrangement), the experimenter would say "Right" and if not, he would say "Wrong."[5] In any event, after a standard interval, the next stimulus word was presented, and the subject proceeded thus through the entire list. The seemingly paradoxical findings have been described by Thorndike (47) as follows:

> The announcement of "Wrong" in our experiments does not weaken the connection, so far as we can see. Here there is more gain in strength from the occurrence of the response than there is weakening by the attachment of "Wrong" to it. . . . Annoyers do not act on learning in general by weakening whatever connection they follow (pp. 45–46).

This announcement was, of course, diametrically opposed to common observation and to Thorndike's own previously reiterated position. For example, in 1911, he had written (46):

> Of several responses made to the same situation . . . those which are accompanied or closely followed by discomfort to the animal will, other things being equal, have their connections with that situation weakened, so that, when it recurs, they will be less likely to occur. The greater the . . . discomfort, the greater the . . . weakening of the bond (p. 244).

It is not surprising that this reversal on the part of Thorndike precipitated a flood of controversy and stimulated much new research. This work has recently been reviewed by Stone (42, 43), who has reported emphatic confirmation of Thorndike's results. Stephens (40), Tilton (50), and others had questioned Thorndike's findings on methodological grounds, but Stone believes he has met these objections and has fully substantiated Thorndike's original claims.

However, Stone is well aware of the paradoxical nature of these findings, especially as they relate to the results of animal research. He says (43):

> There is one point on which all reviews of the past decade are in agreement: the

[5] In certain experiments, some stimulus words were "neutral" in the sense that the experimenter said neither Right nor Wrong to the subject regardless of what response he gave to them.

area most in need of research for both factual and theoretical reasons is the influence of punishment (p. 181).

The task of resolving apparently conflicting results in the experimental literature on the effects of punishment is an all but impossible one in the present state of incomplete knowledge (pp. 197–98).

Stone, in his review, makes no reference to the Wischner experiment and could not have known of the present writer's interpretation thereof because of simultaneity of publication. Otherwise, it is possible that he might have expressed himself less pessimistically regarding the prospect of ever resolving the "apparently conflicting results . . . on the effects of punishment." The Muenzinger findings, which Stone does cite, need cause no further perplexity regarding motivation and learning theory—not, that is to say, unless facts are brought to light other than those now available. And it is entirely possible, indeed probable, that further inquiry will reveal "misplaced ingenuity" as likewise responsible for the ambiguous Thorndike-Stone findings. Here are some possibilites.

In the first place, there is no evidence that the word "Wrong," pronounced by the experimenter is really a "punishment," in the sense of involving marked drive increase and the conditioning of fear. It may be an "annoyer," but this is not to say that it is a punisher, properly speaking. Moreover, there is no proof that the experimenter's "Wrong" following a response is any more disagreeable than is his silence. Silence may give "consent" in some circumstances, but it is more likely to be taken as indicating disapproval or reservation. It may therefore be quite misleading to say that punishment, as mediated by the word "Wrong," does not reduce the likelihood of a response below the level of "chance expectation." The truth may be rather that the "punishment" resulting from Wrong is simply no greater than that produced by the experimenter's silence. In this event, all of the nonrewarded responses (Wrong and silence) would be equally punished, and it would not be strange if there were no differentiation in their likelihood of occurrence.

It has been shown by Stone (43) that a "light shock" is no more effective as a punishment than is the spoken word, "Wrong." This is in no way remarkable. But let the shock be made actively painful! The subjects will either quit making the responses that are followed by shock, or they will abandon the whole enterprise. Nor should we forget that human subjects sometimes have a conception of themselves which makes them feel that it is unworthy of them to be either deterred or moved by pain. We must not forget that with such subjects we always have the subtleties of ego psychology to reckon with, and that preservation of self-esteem (prevention of guilt and anxiety) may cause a person to "rise above" the influence of both rewards and punishments of a segmental nature. Laboratory ("naive") animals will therefore probably continue to be the most sensitive and reliable indicators of the effects of both reward and punishment, and in such subjects there is little or no ambiguity with respect to the latter phenomenon.[6]

[6] One slightly enigmatic note was struck by Coppock (7) at the 1951 meeting of

But perhaps the greatest single source of current ambiguity with regard to the role of punishment comes from the way in which this topic is treated, or rather neglected, in the systematic writings of Hull. In a 422-page book (13), neither punishment nor fear is regarded as sufficiently important to be listed in the Index. There is a chapter on "Inhibition and effective reaction potential," but it does not go beyond positing a "primary negative motivational state or condition" which is produced, in original form, by the organism's own repetitive activity and which can be evoked, as a conditioned reaction, by stimuli which have preceded or accompanied the unconditioned arousal of this (fatigue?) state or condition.

This is very reminiscent of the theorizing of Thorndike in 1911 (and of Troland, at a later date). In roughly equivalent (but simpler) language, Thorndike assumed that, in respect to the learning process, reward (Hull's ${}_{S}E_{R}$) was "positive" and that punishment (I_R or ${}_{S}I_{R}$) was "negative," each being able to counteract, undo, and reverse the effects of the other.[7] In the first section of this chapter we have already reviewed some of the reasons why Thorndike and others have been forced to abandon the simple algebraic-summation (action-counteraction) conception of reward and punishment, but it may be that something has been missed and that Hull has found the trouble and will make the older formulations work. This, however, does not seem likely. Although he does not explicitly claim complete generality for his system in this respect, yet the title of the chapter cited and reference to "the concept of inhibitory dynamics" (13, p. 283) suggest that he is at least aiming at a theory of wide applicability. He says that the chapter will take "experimental extinction [as] a point of departure," but he does not state that the principles adduced apply only to extinction and not to the certainly closely related phenomenon of punishment.[8]

Hull's treatment of "inhibitory dynamics" leaves many unsolved problems. We have, for example, no answer to the question of what happens when

---

the Midwestern Psychological Association. This investigator reported preliminary findings to the effect that immobilized rats which had been shocked for turning their heads to one side continued to avoid making this response for a time after shock was discontinued, but that they then, for a period of time, turned their heads in the previously punished direction unusually often. It is too early to be sure that this effect is reproducible or that it is not due to an artifact of some sort. But there is reason to think it may be real. Whiting & Mowrer (53) and Berlyne (5) have reported laboratory observations which suggest that when fear of punishment is strong, the effect is to inhibit whatever responses the punishment has previously followed; but that when fear is of comparatively low intensity, it may produce active exploration of ("reality testing," "interest," "curiosity"?), or even a kind of attack upon, the signal or situation with which it is connected.

[7] Much the same type of thinking is inherent in Pavlov's writings. Cf. his reference to the "clashing of excitation and inhibition in the cerebral hemispheres" as the cause of experimental neurosis (32, 33).

[8] Elsewhere (24) the present writer has taken the position that they are essentially the same. See also Mowrer & Jones (27).

a habit is opposed, not by "reactive inhibition" (in either its original or conditioned form), but by the effects produced by a drive such as electric shock. Are its effects likewise converted into a "substance" (like "fatigue") residing in the region of the organism where the shock was administered? And if not this, then what? Nowhere do we find reference to such a commonplace phenomenon as fear, and the chapter seems forced and loose-jointed in other ways.

According to Hull, the only time at which anything happens from the standpoint of learning is at the termination (partial or complete) of a drive. What happens at onset, it appears, is of no consequence. We know, of course, that if a response occurs when a drive is, so to say, "on," that response will indeed be reinforced when the drive goes "off" and that the closer the response is to the drive termination the more it will be strengthened (gradient of reinforcement). Now, if what happens when a drive comes on is of no significance—only what happens when it goes off—then we might reasonably assume that responses which precede the onset of a given drive (having been instigated by some other, antecedent drive) would be in no way influenced by drive onset, only by drive termination; and since drive termination is reinforcing, then the only effect of having a second drive come on and then go off (in the manner of "punishment") after the occurrence of a response would be to strengthen it. The facts indicate that, on the contrary, punishments of even the briefest duration may be strongly inhibitory, and there is no unequivocal evidence that punishments (unless complicated by secondary drive reduction, as in so-called "masochism," for example) ever act in the way suggested by Hull's assumptions. Experiments on punishment which are now nearing completion clearly indicate that the conditioning of fear is the primary consideration and that here it is the *onset* of a drive, and this alone, that is important. The termination of the punishment drive is of only indirect significance, or none at all. Yet it is explicitly stated by Hull in numerous places in his book that all learning is dependent upon the "law of reinforcement" (drive reduction). In the light of this type of theorizing, it is not surprising that confusion abounds. Montgomery (22) has recently published a paper, inspired by Hull's formulations, entitled: "An experimental investigation of reactive inhibition and conditioned inhibition." The conclusion:

> Three of the eight predictions [deduced from the theory] are fully confirmed. Theoretical expectations, consistent with Hull's theory, are offered to account for the negative findings (p. 50).

Appetitive Drives and Secondary Reinforcement

What is in some ways a disproportionate emphasis has been put on the problem of punishment in this chapter, with the result that other topics will have to be slighted; but the space devoted to this problem is perhaps justifiable in that there is today so much seemingly unnecessary confusion concerning it. However, there is no lack of real dilemmas in the field of

motivation theory, and in this section we shall briefly sketch one of the most interesting of these.

There has long been a paradox with respect to the conditioning or sign learning connected with drive reduction. We know that such a state of affairs will "stamp in," or "reinforce," actions which shortly precede or are contiguous with it, and it is clear that stimuli (both exteroceptive and proprioceptive) which are associated therewith take on special significance, but the precise nature of this significance, or meaning, has not been clearly established. Several years ago, Fletcher (9) demonstrated that if chimpanzees are shown a large piece of food which is then hidden in a cup located on top of a weighted box, they will pull harder on a piece of rope attached thereto than if the piece of food exhibited to them and then put into the cup is small.

Here we seem to have unequivocal evidence that the reaction aroused by the sight, or sign, of food is a drive; and there are many everyday observations to support such a view. On the other hand, there is a long line of researches which shows that stimuli which have been associated with eating and other forms of drive reduction take on rewarding properties. One of the first laboratory demonstrations of this effect is that of Williams (54), who was able to show that if rats have been repeatedly fed in a particular little box, such rats, when hungry, are apparently reinforced by finding the box, though empty, at the end of a maze, much as they would be by food itself. If the essential nature of reward is some form of drive reduction, we therefore have seemingly contradictory facts: a stimulus associated with primary drive reduction leads to an increase in drive (i.e., secondary, appetitive motivation); a stimulus associated with drive reduction leads to a decrease in drive (satisfaction, or secondary reinforcement).[9] To the best of this writer's knowledge, no one has to date fully resolved this inconsistency. Miller (20), in his 1951 survey of research on "Learnable Drives and Rewards," has the following to say with respect to the problem:

> Experiments in which pain is used to establish fear have illustrated the functions of learnable drive; those in which hunger and food are the primary drive and reward point up the role of learned reward, or, as it is often called, secondary reinforcement (p. 454).
>
> . . . . . .
>
> In the foregoing experiments [by Wolfe] the chimpanzees worked in order to receive a token they could see and did not work when they saw the apparatus was empty. In other words, the sight of the token served as an incentive. Wolfe also ob-

[9] Hull (13) has defined "secondary reinforcement" as "primary reinforcement" which has become conditioned to a sign or signal of some sort. For reasons which are reviewed elsewhere (24, chapter 12), the present writer believes this formulation to be entirely unsatisfactory. More justifiable is the hypothesis that secondary reinforcement is simply the reinforcement which occurs when a secondary drive is reduced. The contradiction referred to above can therefore be phrased thus: How can we explain the fact that signs associated with primary drive reduction seem sometimes to acquire motivational (appetitive) functions and sometimes rewarding (reinforcing) properties?

served that after the token had acquired reward value the sight of it caused the animal to perform without further training habits previously established on the basis of food (p. 460).

. . . . . .

According to the drive-reduction hypothesis, stimulus situations acquire drive value if responses that increase stimulation are reinforced by a subsequent reduction in the total strength of drive. They acquire reward value if responses that reduce stimulation are reinforced. It is impossible for the same situation to be a drive and reward simultaneously, but these two effects could be produced in rapid succession or in slightly different contexts. For example, entering a food box could first serve as a learned reward, and then, if food were not forthcoming, it could arouse learned drive. Furthermore an increase in external stimulation may produce a reduction in total stimulation, as when a fairly strong light seen by a person lost at night reduces a very strong fear.

Other theories of reinforcement (contiguity, confirming reactions, pleasure, or expectancy) might allow the same stimulus to act simultaneously as drive and reward. To date there are no experiments that indicate whether a given stimulus can act simultaneously as a learned drive and reward or that show the conditions that determine whether it will act as one or the other (pp. 463–64).

It is suggestive in this context to examine briefly the psychology of money. Like Wolfe's tokens, it manifestly has both incentive and reward value; but as Miller asks, How can it have both functions simultaneously? Perhaps the answer is that it does not; perhaps it is an incentive, i.e., arouses secondary drive, when it is seen as potentially available (if you work for it) but not yet possessed and that the reward function comes only when it is "in hand." However, this interpretation—plausible as it may be from an introspective standpoint—does not fully accommodate all the experimental facts and must remain a matter for further inquiry.

But in one respect secondary reinforcement theory has begun to produce unequivocal experimental results and to bear fruit practically. In a study reported during the past year by the writer (24), it has been shown that the initial steps in the learning of language (and perhaps other "imitative" actions) in both human infants and "talking birds" are apparently dependent upon the following mechanism. Good mothers, and good bird trainers, characteristically vocalize a great deal as they care for their "babies," with the result that the sounds they make come to "sound good" to their charges; and when the latter, in the course of more or less random vocalization, happen to make similar sounds, they immediately experience secondary reinforcement, i.e., they experience (by generalization) a part of the comfort and reassurance (drive reduction) provided by the sounds produced by the caring, mothering person. This "autistic" conception of language learning has numerous implications, especially for the understanding of identification, play, fantasy, and probably certain forms of art.

In a paper entitled, "Mechanisms of Learning," the Polish investigator, Kornorski (15), has provided a striking confirmation of the foregoing interpretation. In the language-learning situation, the stimulus (vocalization) which is associated with primary reward is first produced by another or-

XUM

ganism. Later, the infant (or bird) "does it himself," and experiences some of the same secondary reinforcement. Kornorski points out an interesting variation of the same mechanism. He observes that if the forepaw of a hungry dog is passively lifted and if the dog is then given food, the dog soon begins to make this response actively. This, of course, is nothing more than the familiar trick, known to every farm boy, of teaching a dog to "shake hands," but it is nonetheless instructive. If the proprioceptive and other stimuli associated with passive leg flexion are made to "feel good" by being associated with hunger reduction, then when the animal, in the course of "random" activity, subsequently happens to lift the leg, these same stimuli will again occur and will be followed by secondary reinforcement.

As in the case of language learning, the autistic satisfaction experienced in connection with such a response will, of course, tend to undergo extinction (i.e., to "wear out"); but it may be enough to get the response to occurring and it can then be directly reinforced, by food, social approval, or the like. That is to say, once a response (vocal, motor, or whatever) has developed a little "habit strength" on the basis of autistic satisfaction, it can then become instrumental in the more objective sense of the term and be established as part of the organism's permanent behavior repertoire.

Since the foregoing interpretation of language learning was arrived at only after the author had adopted a consistent two-factor theory of learning (23), it is noteworthy that Kornorski (16) was one of the first two-factor theorists and is continuing (despite awkward terminology) to differentiate sign learning and solution learning in his thinking. He says:

> If we assumed for the moment that Pavlovian conditioned reflexes (both excitatory and inhibitory) were the basis of *all* acquired behaviour of the animal, then this behaviour should be described in the following way.
>
> . . . . . .
>
> If we take into consideration the whole of animal behaviour and especially the motor activity, we shall observe at once that it cannot be confined in this scheme. For we know very well that the motor activity is not less modifiable than the receptive activity, that it also changes as the effect of the animal's experience, and cannot in the least be reduced to the mere compounds of unconditioned responses. Therefore, besides the Pavlovian conditioned reflexes [conditioning, type I] there must be other mechanisms of plasticity which would account for these other forms of acquired behaviour.
>
> In 1928 we succeeded in separating out a new form of conditioned reflex, different from the Pavlovian conditioned reflex, which we called the "conditioned reflex of the second type" (Miller and Kornorski, 1928). On the basis of our experimental work concerning this type of reflex (vide Kornorski, 1948, chapter XI) the principles of its elaboration can be generally formulated as follows . . . (pp. 417–18).

Although his terminology is needlessly complex and clumsy, it is clear that by the "conditioned reflex of the second type" Kornorski means nothing more than problem-solving behavior of the kind emphasized by Thorndike, Hull, and many others. After giving the example of the dog which, following passive flexion of the forepaw and feeding, learns to make this response himself as a means of obtaining food, Kornorski continues:

According to the definition, in order that any movement may become the effect of a conditioned reflex, second type, the proprioceptive stimulus generated by its performance must become a conditioned signal of a certain unconditioned stimulus [conditioning, type one]. Therefore, if a given movement is not accompanied by any [such sensory] reception, as happens, for instance, in the case of de-afferentation of the limb, then the flexion of this limb cannot become a conditioned stimulus of the first type, and consequently the animal is unable to learn to perform this movement as the effect of the second type of conditioning, unless the proprioception can be substituted by other stimuli, e.g., visual, as in the case of people suffering from tabes dorsalis. . . .

On the other hand, the problem arises whether all the movements accompanied by proprioception, or, more generally, all activities which are "felt" [including, that is to say, the emotions] can become conditioned responses of the second type. As to the autonomic activities there is no doubt whatever that they cannot. If we evoke the erection in a dog (or for that matter in man) and reinforce it, say, by food, we shall not teach the animal to "perform" erection, as we should teach him to perform a movement. We are not able to turn pale or to blush purposely (i.e., in order to reach some positive aim or to avoid some negative result) in the same way as we are able to move our limbs, although both these effects are accompanied by sensation (pp. 419–420).

And later Kornorski remarks:

Now we may ask again whether these two types of conditioning cover all animal behaviour, or whether there are other elementary mechanisms of learning not reducible to these two.

We have a good deal of psychological evidence indicating that there is a special kind of behaviour [note that it is not called "learning"] based on "reasoning" or "insight" and called "intelligent behaviour" which is claimed to be distinct from habit behaviour and is often contrasted with it (p. 422).

The derivation of insight which follows this quotation is almost exactly the same as that put forward independently by the present writer in 1950 in a paper entitled, "An Experiment Exemplifying 'Learning' as Induction and Insight as 'Deduction'" (24). These developments cannot be pertinently discussed in the present chapter, but they suggest that behavior theory is today developing at least a semblance of "power" in the sense that basic principles are emerging which, in the hands of independent investigators, lead to the same predictions and deductions.

Motivation and Latent Learning

It now appears to be only a matter of time until the "latent-learning" issue is closed. It has been abundantly demonstrated that living organisms can learn without such gross, primary rewards as reduction of hunger, thirst, or pain; and there is no lack of well-established alternative mechanisms. Secondary drives provide numerous occasions for problem-solving learning, without the operation of primary drives or rewards; and it also seems increasingly probable that sign learning (conditioning proper) can occur on the basis of a strict contiguity (cognition?) principle. Moreover, in a complex organism such as man, where the personality or self-system represents many

integrative resolutions of complicated conflicts, an intense drive state (anxiety) can be experienced without immediate involvement of either primary or secondary drives in the usual sense of these terms. Coupled with this special capacity to be troubled, or driven, is the fact that human beings (and to some extent certain other organisms) carry on an intricate symbolic (thought) life which may likewise result in behavior changes which bear no very close relationship to the immediate situation.

"Learning by doing" is, therefore, by no means a universally applicable formula. At most it is relevant to what has here been called solution (reward) learning; sign (contiguity) learning may be a much less active matter, involving "experience" but not necessarily "doing" in any overt sense; and when the possibilities of response mediation (insight) are considered, the fact that learning is sometime latent (and manifested only in much later performance) is not at all surprising.

Motivation, Teleology, and Servo Mechanisms

One of the criticisms which has dogged motivation theory throughout its history is the charge that is is "too mechanistic." Drive, like steam in a boiler, may have power; but, so goes the argument, it does not have foresight, intuition, altruism, creativity, and other subtle qualities which human beings and, in a lesser degree, even animals display. Man, we are told, is not a machine and his functioning should not be conceptualized on the model thereof. Although man has presumably not become more mechanical in the last half century, machines have certainly become much more "human"; and sometimes it looks as if we may learn to solve some of our theoretical difficulties, somewhat ironically, from machines, rather than from human beings.

Within the year which is here under review, a number of works have appeared which are relevant to this problem; and one of the most penetrating of these is that of the Austrian naturalist, Lorenz, in a paper entitled, "The Comparative Method in Studying Innate Behaviour Patterns" (17). This compact, closely-reasoned paper cannot be at all effectively "summarized," but a few paragraphs will give its general tenor and flavor:

> The conception of the "whole" was to vitalists the very essence of what never could be explained, being the direct consequence of a preternatural factor. Particular stress was laid on the thesis that a whole could never be explained by synthesis of particulate elements. To this fatal resignation of analysis on the vitalistic side mechanists replied by trying to synthetize all the systems of behavior on the basis of too few elements. This "atomism" is an error very characteristic of mechanistic schools. This optimistic attempt was, of course, doomed to failure, but not before it had brought valuable results. Even very crude simplisms have done less damage to the process of science than resignation (p. 224).
>
> . . . It is an everlasting merit of Gestalt psychologists to have been the first consciously to introduce the method of a truly correlative analysis of organic systems (p. 224).
>
> . . . . . .
>
> There is not a single organic system which is really a "Gestalt," nor one that is a

mosaic. Every real organic system is built up on *both* principles, mutual interaction of correlated parts playing a role as well as particulate elements influencing the whole in one-way causation (p. 225).

. . . . . .

Vitalists overrate the "whole" and totally neglect the particulate element, mechanists neglect regulative interaction and overrate those particulate independent elements which they happened to succeed in isolating. Of these errors the mechanistic one is obviously far less detrimental to research than the "Gestalt" psychological one. Linear causal influence . . . *does* exist . . . . On the other hand, linear causal influence leading from the whole to the part, as assumed by those overrating the "Gestalt" principle simply does not exist, any part influenced from the side of the whole always reacts upon it (pp. 226–27).

. . . . . .

As regards wholes or systems, they have a disconcerting way of getting out of order and thereby supply us with a strong motive for wanting insight into their causal texture (p. 228).

. . . . . .

William McDougall has certainly gained very great merit by stressing the importance of spontaneity of behavior as an argument against the chain-reflex theory of the mechanistic schools. . . . Also, his slogan, "The healthy animal is up and doing," which he threw into the teeth of the chain-reflex theory, is worth remembering. But he and other purposive psychologists did not confine themselves to using the fact of spontaneity as an argument against the chain-reflex theory, but regarded it also as an argument against the general assumption that behavior is causally explicable (p. 230).

. . . . . .

If we concede it as a scientifically legitimate sort of proceeding, to bridge any arbitrarily chosen gap in our present understanding of nature by assuming an obliging little wonder, then all scientific research becomes a pure farce (p. 231).

Within recent years, "servo" theory, with its emphasis on the principle of "feed-back," has produced machines which are indeed "almost human" and has helped us appreciate the essentially "mechanical" nature of organic life and action. Drive→activity→drive-reduction is the basic paradigm for biological self-regulation or homeostasis and represents the beginnings of an organic system and of purpose. But whereas a mechanical homeostatic system, such as a simple thermostat, may have only two possible stimuli (cold and hot) and two possible responses (on and off), living organisms, at least the more complex ones, are capable of making many, many responses to a single stimulus, or drive, and can moreover select (through learning) not only the responses which work from those which do not, but those which work best from those which work less well. This is a remarkable feat, but it does not exceed what can, in at least a delimited way, now be done by giant computers and other intricate electronic and/or mechanical "minds."

As Pavlov long ago pointed out and Sherrington (38), before him, had clearly intimated, the fact that living organisms are equipped with a variety of "distance receptors" means that they can react to the merest shadow of "coming events" and, through conditioning, can be put into highly adaptive action anticipatorily, "foresightfully." The only difficulty with the Pavlo-

vian school in this regard was that it tried to make conditioning a simple matter of connecting external stimuli and external responses, without taking sufficient account of emotions as significant intervening variables. Certainly not everything that happens in a complexly interacting system (not even in an inorganic one) need be externally visible! While the essence of consciousness and other mysteries have not yet been plumbed, still there has been a steady decline in the number of "obliging little wonders" which we have to assume to understand behavior, and that number will undoubtedly continue to lessen.

During the year, one of the most dramatic instances of the servo or feed-back principle in human "engineering" was demonstrated by Fairbanks (8a). By recording speech and playing it back to subjects a fraction of a second delayed, he has shown the most remarkable forms of temporary speech (and "personality") disorganization. Distortions of inflection, stuttering, tremor, and other maladaptations are very pronounced (along with more generalized disturbances) and indicate how closely we ordinarily "monitor" our vocal behavior. When we talk or sing we apparently "listen to ourselves" constantly, and there is every reason to suppose that in the course of behavior in general there is an exceedingly complex interplay of motor output and sensory return. When we act, obviously, we stimulate and thus influence, not only others, but also ourselves; and it is within the framework of this general arrangement that the kinds of systems which we call living organisms—and probably also the phenomenon of consciousness—emerge.

By the freakish device of catching the sound which a person makes as he talks (or sings), delaying it slightly, and then returning it to him somewhat "out of phase," it is possible (as in no other sensory-motor system at present) to disorganize well-established mechanisms almost instantly—with generalized psychological consequences which have thus far been studied in only the most preliminary way. Here, in other words, is a highly efficient (and apparently harmless) method of deliberately putting an organic system "out of order" and of studying not only the specific integrations involved in the vocal-aural mechanism, but also, in some degree, in the personality as a whole.

Anxiety and Ego Psychology

Two books, one by May (18) and the other by Rogers (36), high-light developments with respect to anxiety theory and ego psychology during the period under review.[10] One of the outstanding features of the first volume is the author's emphasis upon the distinction between neurotic anxiety and normal anxiety. Heretofore there has been a tendency in clinical circles to think of anxiety as necessarily pathological. Fear, it has been said,

[10] Two other important books should be mentioned, even though merely by name, in this connection. They are the symposia, *Projective Psychology* and *Perception: An Approach to Personality*, edited respectively by Abt & Bellak (1) and by Blake & Ramsey (6).

is the normal reaction to danger, and anxiety is experienced only when the object of one's fears is lost from consciousness, due to repression. This is the gist of Freud's "second theory" of anxiety, and one which has had great influence.

May's position is rather that fear and anxiety are basically different phenomena, fear being a segmental, even a somewhat 'impersonal" type of response, whereas anxiety involves the very essence of one's psychic existence and experience. Consciousness and self-awareness, May implies, are built up, systematized, organized from the resolution and integration of many competing trends and demands within the total personality, and it is when this unified whole is threatened [cf. Snygg & Combs (39)] that the individual experiences anxiety. Fear may be one of the elements in a conflict, but fear is not necessarily associated with conflict and may be experienced as a relatively isolated, non-ego-involving event. Anxiety, on the other hand, always affects, as May repeatedly says, the very core of personality.

Now the distinction between normal and neurotic anxiety is this. Normal anxiety is associated with conflicts wherein all the elements are fully conscious or easily capable of becoming so. Neurotic anxiety, on the other hand, occurs under the following circumstance. First, it is assumed there has been normal anxiety, involving a fully conscious conflict. This conflict has been resolved, not by the normal process of compromise, synthesis, integration, but by the expedient of repression. Now, when subsequently there is a threatened "return of the repressed," the individual may experience acute anxiety but be unable to identify fully its source or meaning. When this happens the anxiety is said to be "neurotic."

On the basis of this type of thinking, the present author (24) has suggested a possible operational distinction between "counseling" and "psychotherapy," the former term applying to techniques designed to help persons deal with problems which produce normal anxiety and the latter implying the use of techniques which are more specifically appropriate to dealing with conflicts which have "become unconscious."

The essence of Rogers' treatment of anxiety and self-pyschology is contained in the concluding chapter of his book (36) and is epitomized in the following excerpt:

> [In summary] this chapter has endeavored to present a theory of personality and behavior which is consistent with our experience and research in client-centered therapy. This theory is basically phenomenological in character, and relies heavily upon the concept of the self as an explanatory construct. It pictures the end-point of personality development as being a basic congruence between the phenomenal field of experience and the conceptual structure of the self—a situation which, if achieved, would represent freedom from internal strain and anxiety, and freedom from potential strain; which would represent the maximum in realistically oriented adaptation; which would mean the establishment of an individualized value system having considerable identity with the value system of any other equally well-adjusted member of the human race (p. 532).

Within the space of a few pages it is manifestly impossible to do justice, either from the standpoint of appreciation or criticism, to Rogers' thoughtful

and gracefully written book. For a more comprehensive appraisal, the interested reader should rely upon his own impressions of this work in the original. However, it is pertinent to note that despite the continuing distinctiveness of Rogers' approach, there are growing indications of convergence in his thinking with that of other leaders in the field and that a stable, relatively unified body of fact and theory with respect to personality disorder and its treatment may now be in the making.[11]

LITERATURE CITED

1. Abt, L. E., and Bellak, L., Eds., *Projective Psychology* (Alfred A. Knopf, Inc., New York, 485 pp., 1950)
2. Allport, G. H., *The Nature of Personality—Selected Papers* (Addison-Wesley Press, Inc., Cambridge, Mass., 220 pp., 1950)
3. Bain, A., *Mental and Moral Science* (Longmans, Green & Co., London, 2 vols., 751 pp., 1879)
4. Bechterev, V. M., *General Principles of Human Reflexology* (International Universities Press, Inc., New York, 467 pp., 1932)
5. Berlyne, D. E., *Brit. J. Psychol.*, **41**, 68–80 (1950)
6. Blake, R. R., and Ramsey, G. V., *Perception: An Approach to Personality* (The Ronald Press Co., New York, 442 pp., 1951)
7. Coppock, H., *Non-avoidance of a Stimulus Associated with Onset of Electric Shock. Symposium II. Conditioned Anxiety* (Presented at 23rd Ann. Meeting of Midwestern Psychol. Assoc., Chicago, April, 1951)
8. Dollard, J., and Miller, N. E., *Personality and Psychotherapy* (McGraw-Hill Book Co., Inc., New York, 488 pp., 1950)

8a. Fairbanks, G., and Jaeger, R., *J. Speech Hearing Disorders*, **16**, 162–64 (1951).

9. Fletcher, F. M., *Effects of Quantitative Variation of Food Incentive on Physical Work by Chimpanzees* (Doctoral thesis, Yale Univ., 1939)
10. Freud, S., *Collected Papers*, **I** (International Psycho-Analytical Press, London, 359 pp., 1924)
11. Freud, S., *A General Introduction to Psycho-Analysis* (Liveright Publishing Corp., New York, 412 pp., 1935)
12. Goldstein, K., *The Organism* (American Book Co., New York, 533 pp., 1939)
13. Hull, C. L., *Principles of Behavior* (Appleton-Century-Crofts, Inc., New York, 422 pp., 1943)
14. Koch, S., *Psychol. Rev.*, **58**, 147–54 (1951)
15. Kornorski, J., in *Physiological Mechanisms in Animal Behavior* (Academic Press, Inc., New York, 482 pp., 1950)
16. Kornorski, J., and Miller, S., *J. Gen. Psychol.*, **16**, 264–72 (1937)
17. Lorenz, K. Z., in *Physiological Mechanisms in Animal Behavior* (Academic Press Inc., New York, 482 pp., 1950)
18. May, R., *The Meaning of Anxiety* (The Ronald Press Co., New York, 376 pp., 1950)
19. Miller, N. E., in *Personality and the Behavior Disorders*, 431–65 (Hunt, J. M., Ed., The Ronald Press Co., New York, 1944)
20. Miller, N. E., in *Handbook of Experimental Psychology*, 435–72 (Stevens, S. S., Ed., John Wiley & Sons, Inc., New York, 1951)

[11] This review, because of space limitations, has had to be selective rather than all-inclusive in its coverage. By way of supplementation, the reader will find Reymert's symposium on *Feelings and Emotions* (34) and selected chapters in Stevens' *Handbook of Experimental Psychology* (41) highly useful.

21. Miller, N. E., and Dollard, J., *Social Learning and Imitation* (Yale Univ. Press, New Haven, 341 pp., 1941)
22. Montgomery, K. C., *J. Exptl. Psychol.*, **41**, 39–51 (1951)
23. Mowrer, O. H., *Harvard Educ. Rev.*, **17**, 102–48 (1947)
24. Mowrer, O. H., *Learning Theory and Personality Dynamics* (The Ronald Press Co., New York, 776 pp., 1950)
25. Mowrer, O. H. (Lecture delivered at Univ. Minnesota, November, 1950)
26. Mowrer, O. H., *Psychol. Rev.* (In press)
27. Mowrer, O. H., and Jones, H., *J. Exptl. Psychol.*, **33**, 369–86 (1943)
28. Muenzinger, K. F., *J. Comp. Psychol.*, **17**, 267–77 (1934)
29. Muenzinger, K. F., and Wood, A., *J. Comp. Psychol.*, **20**, 95–106 (1935)
30. Muenzinger, K. F., and Fletcher, F. M., *J. Comp. Psychol.*, **22**, 79–91 (1936)
31. Muenzinger, K. F., Berstone, A. H., and Richards, L., *J. Comp. Psychol.*, **26**, 177–85 (1938)
32. Pavlov, I. P., *Conditioned Reflexes* (Anrep., G. V., Trans., Oxford Univ. Press, London, 430 pp., 1927)
33. Pavlov, I. P., *Lectures on Conditioned Reflexes* (Gantt, W. H., Trans., International Publishers, New York, 414 pp., 1928)
34. Reymert, M. L., *Feelings and Emotions* (McGraw-Hill Book Co., Inc., New York, 603 pp., 1950)
35. Roberts, W. H., *J. Genetic Psychol.*, **37**, 35–58 (1930)
36. Rogers, C. R., *Client-Centered Therapy* (Houghton Mifflin Co., Boston, 560 pp., 1951)
37. Sherif, M., and Cantril, H., *The Psychology of Ego-Involvements* (John Wiley & Sons, Inc., New York, 525 pp., 1947)
38. Sherrington, C. S., *The Integrative Action of the Nervous System* (Yale Univ. Press, New Haven, Conn., 1906)
39. Snygg, D., and Combs, A. W., *Individual Behavior* (Harper & Bros., New York, 386 pp., 1949)
40. Stephens, J. M., *J. Exptl. Psychol.*, **17**, 536–55 (1934)
41. Stevens, S. S., in *Handbook of Experimental Psychology* (John Wiley & Sons, Inc., New York, 1436 pp., 1951)
42. Stone, G. R., *J. Gen. Psychol.*, **38**, 207–16 (1948)
43. Stone, G. R., *J. Gen. Psychol.*, **42**, 179–224 (1950)
44. Sullivan, H. S., *Conceptions of Modern Psychiatry* (William Alanson White Psychiatric Foundation, Washington, D. C., 1945)
45. Thorndike, E. L., *Biological Lectures from the Marine Biological Laboratory of Woods Hole*, 69–91 (Ginn & Co., Boston, 282 pp., 1899)
46. Thorndike, E. L., *Animal Intelligence; Experimental Studies* (The Macmillan Co., New York, 298 pp., 1911)
47. Thorndike, E. L., *Human Learning* (The Century Co., New York, 206 pp., 1931)
48. Thorndike, E. L., *The Fundamentals of Learning* (Teachers College, Columbia Univ., New York, 638 pp., 1932)
49. Thorndike, E. L., *Man and His Works* (Harvard Univ. Press, Cambridge, Mass., 212 pp., 1943)
50. Tilton, J. W., *J. Educ. Psychol.*, **30**, 95–115 (1939)
51. Troland, L. T., *The Fundamentals of Human Motivation* (D. Van Nostrand Co., Inc., New York, 521 pp., 1928)
52. Watson, J. B., *Psychobiology*, **1**, 51–60 (1917)
53. Whiting, J. W. M., and Mowrer, O. H., *J. Comp. Psychol.*, **36**, 229–52 (1943)
54. Williams, K. A., *Univ. Calif. Pubs. Psychol.*, **4**, 31–55 (1929)
55. Wischner, G. J., *J. Exptl. Psychol.*, **37**, 271–84 (1947)

XUM

AUTHOR INDEX

A

Abbot, F. K., 274
Abel, M. S., 124
Abel, T. M., 6
Aborn, M., 308
Abt, L. E., 289, 296, 297, 303, 305, 435
Ackerman, N. W., 20, 195
Ades, H. W., 96, 247
Adkins, D. C., 397
Adler, F. H., 56
Adolph, E. F., 244
Adorno, T. W., 15, 178, 185, 191, 193, 194
Adrian, E. D., 124, 247
Ahokas, A. J., 119
Albee, G. W., 301, 306
Alberts, 8, 45
Alexander, F., 273, 333, 343
Alexander, T., 16
Alexander, W. M., 400
Allesch, J. von, 166
Allison, A. C., 247
Allphin, W., 228
Allport, G. H., 423
Allport, G. W., 178, 191
Almada, A. A., 324, 334
Almy, T. P., 274
Alsson, L., 120
Altmann, F., 112
Altschule, M. D., 271
Ames, A., Jr., 76
Ames, L. B., 1, 2
Ammons, R. B., 12, 47, 210, 372, 401
Amsel, A., 36
Andersen, A. L., 306, 310, 312
Anderson, A. B., 110
Anderson, G. V., 366
Anderson, K. E., 399
Anderson, R. G., 376
Anderson, T. W., 146, 413
Andersson, B., 120
Andrew, G., 306
Andrew, J. G., 80
Anstey, E., 398
Arensberg, C. M., 186
Armstrong, E. A., 234, 236
Armstrong, T. O., 223
Arnot, R., 261, 264, 265, 266
Arnott, G. P., 249
Arnoult, M. D., 96, 119
Aronson, L. R., 239
Arshavskaia, E. I., 108
Arslan, M., 118
Asch, S. E., 177, 179
Aschan, G. von, 116
Ash, P., 216
Ashby, W., 267
Ashby, W. R., 264
Asher, H., 57, 250
Atkinson, J. W., 269
Attneave, F., 57, 60
Ausubel, D. P., 11
Autrum, H., 243
Aycock, W. L., 278
Azoy, 119

B

Babkin, B. P., 252
Bach, L. M. N., 252
Back, K. W., 180
Backer, R., 39
Baerends, G. P., 234, 235
Baerends-van Roon, J. M., 234, 235
Bailey, C. J., 244
Bailey, P., 255, 263
Bain, A., 419
Baker, R. A., 48
Baker, K. E., 47
Balinsky, B., 352, 354, 365
Ballard, S. S., 59
Banham, K. M., 3
Barahal, G. D., 364
Barat, T., 124
Bard, P., 117
Bare, J. K., 120
Barker, R. G., 21, 22, 180
Barnard, J. W., 249
Barnette, W. L., Jr., 364, 375, 397
Barnouw, V., 11
Baron, M. R., 45
Barrett, D., 226
Barry, J. R., 327, 359
Bartlett, F. C., 178
Bartlett, N. R., 55
Bartlett, M. R., 364
Bartlett, M. S., 138, 412
Bartley, S. H., 112
Baruch, D. W., 6, 324, 335
Bassett, M., 264
Bates, J. A. V., 111
Batterman, R. C., 118
Battersby, W. S., 248
Baumgardt, E., 66
Bavelas, A., 182, 226
Bayer, L. M., 391
Bayley, N., 4, 5, 383, 384
Bayroff, A. G., 219
Bayton, J. A., 120, 121
Beach, B., 225
Beach, F. A., 24, 233, 236
Beardslee, D. C., 73
Bechterev, V. M., 420
Beck, L. H., 122
Beck, S. J., 3, 298
Bedell, B. J., 136
Beecher, H. K., 107, 248
Beets, G. J., 122
Beier, E. G., 7
Békésy, G. von, 86, 94, 98, 99
Bell, E., 10
Bell, G. B., 184
Bell, R. Q., 147
Bellak, L., 289, 296, 297, 298, 302, 303, 305, 307, 372, 435
Bellak, S. S., 307
Beller, E. K., 22
Belman, H. S., 144, 396
Benarick, S. J., 312
Bender, M. B., 248, 249, 250
Bendig, A. W., 41
Benjamins, J., 9, 359
Bennett, G. K., 133, 145
Bennett, S. M., 385
Benson, S., 198
Bentley, C. H., 219
Beranek, L. L., 88
Berdie, R. F., 365, 375, 397
Berenda, R. W., 10
Berger, C., 71
Bergmann, G., 31, 32, 33
Berlyne, D. E., 427
Bernberg, R. E., 200
Berry, R. N., 71
Bersh, P. J., 38
Berstone, A. H., 424
Bertalanffy, L. von, 170
Biesele, R. L., Jr., 228
Bills, R. E., 303
Bilodeau, E. A., 45
Bilodeau, I. M., 309
Bing, H. I., 109
Birch, H. G., 48, 242
Birch, L. B., 403
Bird, R. M., 106, 109
Birnbaum, Z. W., 145
Birren, J. E., 311, 312
Birukow, G., 243
Bitterman, M. E., 30, 37, 41, 43, 73, 77
Bittner, R. H., 218
Blaesser, W. W., 352
Blake, R. R., 167, 171, 287, 302, 435
Blanchard, B. E., 400
Blau, T. H., 339, 364
Bleeker, J. D. J. W., 113
Bligh, H., 14

Block, J., 140
Block, J(ack), 194
Block, J(eanne), 194
Blomquist, B. L., 1
Blum, G. S., 164, 290
Blum, J. S., 247, 249
Blum, L. H., 21
Blum, M. L., 352, 354, 365
Blum, R. A., 247, 249
Blumberg, E., 308, 375
Boddie, J., 219
Bond, G. L., 398
Bonin, G. von, 245, 255
Booman, W. P., 198
Bordin, E. S., 328, 341, 352, 363
Bordley, J. E., 88
Borg, W. R., 398
Borison, M. L., 117
Bors, E., 106
Borstelmann, L. J., 304
Botwinick, J., 312
Bouman, M. A., 66
Bourne, G. H., 123
Boycott, B. B., 252
Boyd, W. C., 120
Brach, F. A. 242
Bradway, K., 331, 353
Braly, K. W., 193
Brammer, L. M., 364
Braun, H. W., 254
Brendler, S. J., 252
Brillouin, L., 170
Brody, M. B., 171
Brogden, H. E., 143, 145, 217
Brogden, W. J., 29, 30
Broida, D. C., 7, 306, 390
Brokaw, L. D., 144
Bromiley, R. B., 107, 247
Bromley, A., 405
Bronfenbrenner, U., 153, 168
Brookhart, J. M., 96, 247
Brower, D., 296
Brower, E., 389
Brown, C. W., 143, 214
Brown, D. D.,
see Denny-Brown, D.
Brown, G. G., 302
Brown, G. L., 394
Brown, J. L., 397
Brown, J. S., 45
Brown, M. A., 312
Brown, P. K., 66
Brown, R. A., 228
Brown, U., 38
Brown, W. L., 37, 48, 241
Browne, C. G., 225
Brožek, J., 241, 268
Bruck, G., 250
Bruner, J. S., 167, 177, 287, 358
Brunswik, E. F.,
see Frenkel-Brunswik, E.
Bryan, J. G., 146
Buck, G. B., 80
Buck, J. N., 295
Bühler, C., 290
Bullock, D. H., 42
Bunch, M. E., 44
Burke, J. H., 219
Burks, B. S., 278
Burns, D. G., 404
Burt, C., 138, 140, 412
Burt, C. L., 160, 165
Burt, D. C., 397
Burton, H. W., 286
Buss, A. H., 49
Butler, M. N., 210, 372
Butterworth, R. F., 3, 15
Buxton, C. E., 30, 39, 140
Buytendijk, F. J. J., 43

C

Cairns, H., 251
Caldwell, W. E., 30
Callahan, R., 293, 371
Calvin, J. S., 285, 371
Cameron, D. E., 354, 355, 357, 365
Cameron, N., 261, 287, 342
Camisasia, L., 118
Campbell, A., 179, 187, 189
Campbell, D. T., 179
Campbell, E. N., 117
Campbell, J. T., 136
Cannicoot, R. G., 309
Canning, W., 373
Canter, A. H., 308, 311
Canter, R. R., Jr., 220
Cantril, H., 192, 196, 423
Caplan, G., 336
Cardullo, H. M., 120
Carl, L. J., 22
Carle, A., 118
Carli, R. A., 364
Carmichael, L., 24
Carp, A. L., 288
Carp, F. M., 296, 370
Carr, E. R., 211
Carter, G. C., 388, 405
Carter, H. D., 400
Casperson, R. C., 66, 75
Cattell, R. B., 164, 171, 183, 191, 200, 295, 374
Causeret, J., 121
Centers, R., 3, 12, 202, 189
Challma, R. C., 283
Chang, H.-T., 70, 248, 256
Chapanis, A., 55, 227
Chapman, W. P., 107, 248
Château, J., 386
Chatham, G. N., 58
Chinn, H. A., 89
Chow, K. L., 74, 247, 249, 250
Christenson, J. M., 215
Citron, A. F., 195
Clark, B., 96
Clark, G., 242
Clark, J. H., 310
Clark, K. B., 3, 13
Clark, K. E., 198
Clark, M. P., 3, 13
Clark, W. E. L. G.,
see Le Gros Clark, W. E.
Clausen, J., 106, 107, 248
Clausen, J. A., 188, 199, 410, 411
Clayton, F. L., 42
Coakley, J. D., 226
Coate, W. B., 37, 43
Cochran, W. G., 414, 415, 416
Coffee, H., 363
Cohen, J., 40, 305, 311
Cohn, C. H., 348
Colby, A. N., 226
Cole, D., 305, 372
Cole, J., 251
Collier, R. M., 328
Collins, M. E., 195
Combs, A. W., 339, 436
Condon, E. U., 67
Cook, P. H., 226
Coombs, C. H., 411
Coppock, H., 426
Corey, D. Q., 146, 375
Corsini, R., 309
Corso, J. F., 95
Cottingham, A., 304
Cottle, W. C., 208
Cottrell, C. L., 77
Cottrell, L. S., Jr., 187, 189
Cotzin, M., 96, 309
Cowen, E. L., 339
Cox, G. M., 414, 415
Critchley, M., 250
Cross, O. H., 211
Crossley, 198
Crowder, T. H., 9
Crown, S., 171
Crozier, W. J., 65
Cruickshank, W. M., 7, 306, 390
Crum, J., 37
Crump, S. L., 415, 416
Culp, S. S., 382
Cureton, E. E., 144

D

Daanje, A., 234, 235
Dailey, J. T., 133, 137
Dallenbach, K. M., 96, 115, 122
Dalton, M., 223, 225
Daniel, W. J., 35
Daniels, H. W., 206
Darley, J. G., 366
Dattman, P. E., 49
Davage, R. H., 71
Davey, L. M., 274
David, H. P., 305
Davidson, H. R., 68
Davis, A., 131
Davis, F. H., 276
Davis, H., 13, 86, 87, 91, 97, 101

XUM

Davis, J. E., 348
Davis, K., 225
Davis, M. K., 216
Deane, M. A., 310
DeCillis, O. E., 373
Dederding, D., 118
Deemer, W. L., 144
Deering, I. D., 111
Deese, J., 31, 43
Degan, J. W., 138, 139, 209
Deinse, J. B., 118
de Kleyn, A. P. H. A., 116, 117, 118
Denes, P., 94
Dengrove, E., 329
Dennegar, L. S., 364
Dennis, W., 2, 171, 287
Denny-Brown, D., 245
De Porte, E., 278
Deri, S. K., 305
Derner, G. F., 308
Dethier, V. G., 119
Deutsch, J. A., 96
Deutsch, M., 195
Devereux, G., 11
De Vinney, L. C., 187, 189
De Vries, H., 113, 114, 115
Dicks, H. V., 194
Diethelm, O., 276
Di Giorgio, A., 116
Dijkgraaf, S., 244
Dimmick, F. L., 62, 63
Dinsmoor, J. A., 42
Doll, E. E., 391
Dollard, J., 3, 24, 322, 333, 344, 345, 352, 354, 355, 357, 422
Dombrose, L. A., 194, 195
Donahue, W., 24
Doppelt, J. E., 133, 398
Dörken, H., Jr., 373
Dose, R. F., 398
Douglas, A. G., 210
Downie, N. M., 402
Dressel, P. L., 360
Drucker, A. J., 402
Dubo, S., 7
DuBois, P. H., 143, 213
Dubrovner, R. J., 300, 370
Duffy, E., 166
Dugan, G. E., 217
Dugan, W. E., 352
Duguid, M., 398
Duisberg, R. E. H., 266
Dukes, W. F., 76
Dulsky, S. G., 212
DuMas, F. M., 286
Duncan, C. P., 44, 71
Duncan, D. R., 55, 58
Dunsmore, R. H., 252
Dymond, R. F., 359

E

Eames, T. H., 402
Ebaugh, F. G., 109
Ebaugh, F. G., Jr., 106, 109
Ebeling, E., 43
Edgerton, H. A., 220
Edwards, A. L., 410, 415
Edwards, M., 14, 194
Edwards, W., 105
Egan, J. P., 89, 90
Ehrenfreund, D., 8, 45
Eibesfeldt, I. E., see Eible-Eibesfeldt, I.
Eibl-Eibesfeldt, I., 237, 238
Eisenberg, P., 89
Eisler, F. G., see Goldman-Eisler, F.
El-Baradi, A. F., 123
Ellerbrock, V. J., 56
Ellis, R. W., 302
Ellson, D. G., 241
Elmgren, J., 393
Elsom, K. O., 267
Elton, C. F., 360
Ely, J. H., 215
Enneis, J. M., 347
Eppel, E., 404
Ergene, S., 243
Erhart, E. A., 125
Erickson, T. C., 247
Erikson, E. H., 11, 191, 342
Erlick, D., 253
Eron, L. D., 290, 293, 303, 304, 371
Esmond, W. G., 118
Estes, S. G., 307
Evans, R. M., 60
Evans, R. N., 144, 396
Ewers, D. W. F., 402
Eysenck, H. J., 139, 152, 154, 160, 161, 162, 164, 166, 171, 199
Ezriel, H., 335, 343

F

Faigin, H., 22
Fairbanks, G., 435
Farnsworth, D., 61, 69, 77
Farr, J. N., 226
Fassett, K. K., 373
Fay, L. C., 398
Feather, D. B., 373
Fedotov, Ü. P., 108
Feifel, H., 3, 396
Fein, L. G., 301, 370
Feingold, L., 278
Feldman, J. B., 64
Fenichel, O., 338
Ferguson, L. W., 220
Fermin, H., 118
Fernandez, C., 86, 87
Festinger, L., 180, 182
Fiedler, F. E., 326, 327, 334, 338, 360, 361, 362
Fields, H., 210
Finan, J. L., 34
Fink, M., 250
Fink, R., 198
Fishback, J., 72, 250
Fisher, J. D., 249
Fisher, N., 198
Fisher, R., 306
Fisher, R. A., 152
Fisher, S., 302, 306
Fisher, V. E., 354, 365
Fiske, D. W., 162, 163, 166, 217, 285, 367, 371
Fitch, M. L., 402
Fitzgerald, D. C., 7
Flanagan, J. C., 142, 215
Flanagan, J. L., 97
Flanigin, H., 252
Fleetwood, M. F., 276
Fleming, G. W. T. H., 171
Fletcher, F. M., 420, 424, 429
Fletcher, H., 87, 88, 89
Flowerman, S. H., 16
Foa, U. G., 199
Folch, J., 267
Ford, A., 227
Ford, A. H., 212
Ford, C. S., 11, 24, 190, 236
Ford, M., 298
Ford, R. N., 199
Forster, G., 116
Fortier, C., 271
Fortunato, V., 122
Fosberg, I. A., 305, 372
Foss, B., 252, 253
Foster, D., 121, 122, 124, 125
Foster, H., 47
Foulds, G. A., 398
Fox, C., 311
Frandsen, A. N., 375
Frank, L. K., 383
Frankenhauser, B., 125, 255
Fraser, A. M., 117
Freedman, M., 363
Freeman, F. N., 160
Freeman, M. J., 312
Freeman, W., 261, 263
French, E. G., 292, 311
French, R. L., 184, 292, 306
French, T. M., 342, 343
Frenkel-Brunswik, E., 15, 167, 178, 185, 191, 193, 194, 287
Freud, S., 419
Freudenberg, R. K., 252, 256
Freyer, D. H., 220
Frick, F. C., 50
Friedenberg, E. Z., 363
Frisch, K. von, 119, 239
Froelich, C. P., 352
Fromm, E., 191
Fromm-Reichmann, F., 323, 332, 342, 346
Fruchter, B., 138, 139
Fujisaki, S., 118
Fuller, P. R., 241
Fulton, J. F., 252, 261, 274

Furst, E. J., 143, 400, 405

G

Gaensler, E. A., 108
Gage, N. L., 179
Gagné, R. M., 47
Gainer, J. M., 119
Galambos, R., 85, 86, 91, 92, 97, 98
Galdston, I., 352
Gales, R. S., 87, 89
Gallagher, J. J., 309
Gamm, S. R., 277
Gandet, F. J., 364
Garceau, O., 176
Gardner, M. B., 87, 92
Garner, W. R., 62
Garrett, H. F., 399
Gastaut, H., 256
Gavrel-Szmanski, M., 121
Gaylord, R. H., 136
Gellhorn, E., 111, 245, 270
Genn, G., 24
Genn, M. M., 24
Gentry, G., 254
Gentry, G. V., 48, 241
Gentry, L., 405
Gerboth, R., 308, 375
Gernandt, B. E., 86, 113, 115
Gesell, A., 1, 381
Gewirtz, J. L., 22
Ghiselli, E. E., 143, 214
Giaccai, F., 118
Gibby, R. G., 287
Gibson, J. J., 56, 60
Gilbert, A. R., 170
Gilbert, C., 208
Gilbert, G. M., 193, 195
Gilbert, W. M., 71, 363
Gilhooly, F. M., 310
Gill, M. M., 347
Girshick, M. A., 411
Glass, G. B. J., see Jersey Glass, G. B.
Glassey, W., 386
Glazer, N., 201
Glees, P., 251, 252, 256
Gleicher, D. B., 179
Gleser, G. C., 143
Glueck, E., 141, 142, 144, 155
Glueck, S., 141, 143, 144, 155
Goetzel, F. R., 119
Goff, R. M., 14
Gold, B. H., 88
Golden, J. M., 328
Goldenberg, H. C., 307
Goldenberg, S., 1
Goldman, F., 18
Goldman, I., 399
Goldman, R., 306
Goldman-Eisler, F., 158, 164
Goldring, S., 256
Goldstein, K., 423
Goldston, E., 330
Golla, F. L., 171
Gomberg, W., 215
Goode, C. E., 225
Goodell, H., 105, 106, 248
Goodenough, F. L., 24
Gordon, I. J., 363
Gordon, M. S., 133
Gorer, G., 11, 190
Gorlow, L., 7, 362
Gottlieb, B., 224
Gottlieb, J. S., 325
Gough, H. G., 14, 194, 312, 373
Gould, W. J., 118
Grace, H. A., 193
Gragg, D. B., 133, 137
Graham, D. T., 106, 248, 276
Graham, H. B., 144
Granit, R., 111
Grant, D. A., 37, 49
Grant, M. Q., 300
Grassé, P. P., 240
Gray, J. S., 78, 216
Graybiel, A., 96, 112
Grayson, H. M., 284
Green, B. F., 415
Green, J. R., 266
Green, M., 250
Green, M. E., 373
Greenberg, R., 46
Greenblatt, M., 261, 264, 265, 266
Greulich, W. W., 384
Grice, G. R., 43, 45
Grieber, M. F., 267
Griffith, J. W., 228
Griffiths, W. J., Jr., 242
Grinker, R. R., 277
Groen, J. J., 112, 114
Grossman, M. I., 244
Grotjahn, M., 324, 336
Grunes, W. F., 179, 200
Guedry, F. E., 117
Guertin, W. H., 304, 308, 310, 352
Guest, L., 198
Guetzkow, H., 175, 182, 223
Guhl, A. M., 238
Guilford, J. P., 64, 66, 133, 138, 139, 376
Guillot, M., 123, 124
Guion, R. M., 221
Gulliksen, H., 134, 142, 143, 409, 410
Gurvitz, M. S., 309, 310
Guth, S. K., 78
Guthrie, G. M., 312
Gutner, L. B., 118
Guttman, L., 136, 188, 199, 410, 411
Guy, W., 310

H

Habgood, J. S., 109
Haigh, G., 324, 331, 353
Hailman, H., 270
Haimowitz, M. L., 195
Haimowitz, N. R., 195
Haines, M. S., 389
Haire, M., 179, 200
Hake, H. W., 37, 89
Haldane, J. B. S., 278
Hallpike, C. S., 92
Halstead, W. C., 261
Hamberger, C. A., 115
Hamilton, C. E., 46
Hamilton, C. H., 143, 412
Hamlin, H., 254
Hamlin, R. M., 301, 306
Hammelburg, E., 118
Hamuy, T. P., 247
Hamwi, V., 262
Handlon, J. H., Jr., 31, 32, 33
Hanes, R. M., 61
Hanvik, L. J., 306, 312
Harding, J., 195
Hardy, J. D., 105, 106, 108, 109
Hardy, L. H., 74
Hardy, W. G., 88
Harlow, G., 373
Harlow, H. F., 29, 30, 31, 39, 42, 44, 48, 241, 249, 253
Harlow, M. K., 39, 241
Harney, M., 162
Harper, A. E., Jr., 293, 310
Harper, R., 228
Harriman, M., 392
Harriman, P. L., 392
Harris, C. W., 394
Harris, D. B., 7, 9, 14, 24, 194
Harris, F. J., 229
Harris, I. D., 7
Harris, J. D., 87, 92, 94
Harris, R. E., 284
Harrison, R., 163
Harrower, M. R., 284, 297, 307, 372
Hartley, H. O., 415
Hartley, P. H. T., 234, 236
Hartridge, H., 70
Hartwell, S. W., 306
Hasegawa, T., 118
Haslerud, G. M., 8
Hathaway, S. R., 293, 312, 373
Hauty, G. T., 114
Hawkins, J. E., Jr., 91, 92
Hay, E. N., 144, 147, 213, 216
Hayes, C., 241
Hayes, K. J., 241
Hayes, S. P., Jr., 176
Hazouri, L. A., 108

Head, H., 264
Heathers, G., 3, 20
Hebb, D. O., 171, 261
Hecht, S., 65
Hedlun, J. M., 95
Hein, G., 239
Heise, G. A., 95
Heiss, R., 163
Heist, A. B., 200, 374
Heist, P. A., 200, 374
Helmick, J. S., 47
Helson, H., 60, 61, 64, 68
Hemphill, J. K., 183, 185, 187
Henry, F. M., 416
Henry, J., 20
Henry, W. E., 210
Henrysson, S., 138, 412
Henschel, A., 241, 268
Hensel, H., 108
Herman, M., 109
Heron, W. T., 30, 347
Hersh, J. H., 121, 125
Herzig, S. A., 210, 372
Hess, E. H., 58, 76, 240
Hess, W. R., 251
Heston, J. C., 133
Heuyer, C., 394
Hewson, L. R., 310
Heyns, R. W., 182
Higham, T. M., 178
Higke, R. V., 267
Hildreth, G., 24
Hilgard, E. R., 30, 287, 344
Himes, H. W., 87
Himmelweit, H. T., 154, 166, 171, 386
Himwich, H. E., 118, 267
Hinkle, L. E., Jr., 275
Hire, A. W., 297
Hirsch, I. J., 85, 90, 92, 93, 94, 97
Hites, R. W., 179
Hoagland, H., 271
Hobbs, N., 333, 334, 362
Hoch, P. H., 171, 267
Hochberg, J. E., 73
Hofstetter, H. W., 76
Hollenberg, E., 9, 20, 22
Holley, J. W., 140
Holmes, F. J., 213
Holmes, R. M., Jr., 223
Holst, E. von, 244
Holt, L. E., Jr., 120
Holt, R. R., 305
Holtzman, W. H., 299, 369, 370
Holzberg, J. D., 302, 306, 310
Holzinger, K. J., 160
Homans, G. C., 186
Honzik, M. P., 4
Hood, J. D., 91, 95
Hopkins, L. T., 400
Hopkins, O., 225
Horenstein, B. R., 35
Hornseth, J. P., 37
Horst, L. van der, 253
Horst, P., 146
Hotchkiss, S. N., 226
House, B. J., 35
Hovland, C. I., 30
Howard, A. R., 311
Howes, D. H., 177
Huang, I., 2
Hubbard, R., 66
Hudson, A. E., 11, 190
Hudson, B. B., 241
Huggins, W. H., 99
Hughes, R. H., 44
Hughes, R. M., 164, 294
Hulk, J., 116, 118
Hull, C. L., 29, 35, 427, 429
Humm, D. G., 213
Humm, K. A., 213
Humphreys, L. G., 400
Hunt, C. C., 110
Hunt, H. F., 283
Hunt, J. M., 328
Hunt, R. W. G., 68, 70
Hunt, W. A., 286, 292, 311
Hunter, J., 256
Hurvich, M., 60
Huston, P. E., 325
Hutt, M. L., 287, 306
Hyman, H., 188, 198
Hymovitch, B., 180

I

Ilg, F. L., 2
Ingalls, T. H., 278
Irwin, O. C., 1
Israel, H. E., 49
Ittelson, W. H., 76
Ives, V., 300
Izard, C. E., 7, 306, 390

J

Jackson, I. J., 110
Jacobs, M. B., 119
Jacobs, W., 240
Jacobson, E., 77, 187, 189
Jacobson, H., 67, 85
Jaeger, R., 435
Jager, H. A., 352
Jahoda, M., 195
James, H. E. O., 14, 193
Jameson, D., 60
Janis, I. L., 187, 189, 254, 271
Jarrett, R. F., 416
Jasper, H., 251, 255, 256
Jefferson, G., 261, 266
Jenkins, W. L., 108, 227
Jenkins, W. O., 42
Jersey Glass, G. B., 273
Jervis, F. M., 8
Joffe, N. F., 6
Johansson, G., 77
John, E., 160
Johnson, D. A., 111
Johnson, G., 3, 13
Johnson, G. O., 7
Johnson, H. G., 143
Johnson, N., 324, 347
Jokl, R. H., 343
Jones, F. N., 70, 214
Jones, L. V., 138, 139
Jones, L. W., 394
Jones, M. C., 4, 216, 384
Jones, M. H., 70, 208
Jones, W. R., 132
Jongkees, L. B. W., 114, 115, 116, 118
Jordan, E., 399
Jost, H., 300, 370
Judd, D. B., 56, 58, 59, 68, 70
Jung, R., 114, 117

K

Kaada, B., 70, 248
Kaada, B. R., 274
Kadis, A. L., 306
Kahana, L., 86
Kahn, R. L., 187, 189, 250
Kallmann, F. J., 278
Kamm, R. B., 363
Kanner, J. H., 33
Kaplan, E., 2
Kaplan, G., 363
Kardiner, A., 190
Karsh, B., 221
Kates, S. L., 213, 222, 301, 389
Katona, G., 176
Katz, B., 111
Katz, D., 180, 182, 187, 189, 193
Katzell, R. A., 144
Kaufman, M. R., 273
Kaufman, P., 329
Kearney, C., 403
Keats, A. S., 107, 248
Keister, M. E., 9
Kell, B. L., 324, 331, 353
Kelley, H., 180
Kelley, H. H., 179, 180, 289
Kelly, E. L., 162, 163, 166, 285, 367, 371
Kelsey, P. A., 92
Kempinsky, W. H., 115
Kendall, P. L., 196
Kendler, H. H., 33, 35, 36
Kendler, T. S., 45
Kennard, M. A., 106, 278
Kennedy, R., 11, 190
Kenney, J. J., 398
Kephart, N. C., 215
Kern, F., Jr., 274
Kerr, W. A., 112, 224, 228, 229
Keys, A., 241, 268
Kimball, A. J., 299, 300
King, H. E., 106, 107, 248

Kistiakowsky, G. B., 123
Kitay, J. I., 305
Kitt, A. S., 179, 188, 189
Kitzinger, H., 308, 375
Klebanoff, S. G., 292
Klein, G. S., 167, 177, 178, 287
Kleyn, A. P. H. A. de, see de Kleyn, A. P. H. A.
Kligerman, S., 331
Klineberg, O., 189, 192, 195
Klopfer, W. G., 304
Klumpp, R. G., 90
Knapp, E., 116
Knapp, P. H., 88
Knehr, C. A., 396
Kniazuk, M., 92
Knight, J. F., 216
Knott, J. R., 309
Knox, G. W., 56
Koch, S., 35, 420
Kock, W. E., 97
Koella, W., 116
Kogan, W. S., 294
Köhler, W., 72, 169, 171, 250
Koomen, M., 65
Korchin, S. J., 304
Korner, A. F., 286, 369
Kornorski, J., 430, 431
Krathwohl, W. C., 137
Krayenbuhl, H., 107
Krech, D., 170
Kretschmer, E., 151
Kris, E., 343
Krogman, W. M., 383
Kroh, O., 160
Krohn, B., 18
Krout, M. H., 212
Kruglov, L., 401
Krynauw, R. A., 246, 250
Kryter, K. D., 85
Kuder, G. F., 211
Kuenne, M. R., 8, 45
Kuffler, S. W., 110
Kühn, A., 243
Kuhn, R. A., 107
Kuhns, F., 292, 300
Kuntz, J. E., 70
Kunze, K. R., 221
Kutash, S. B.,

L

Lacey, J. I., 133, 138, 139, 244
Lackum, J. V., 370
Lamoreaux, R. R., 30
Landgren, S., 120
Landis, C., 253, 264
Lane, C. E., 89
Langhorne, M. C., 40
Lansdell, H. C., 253
Larramendi, M. H., 246
Larsell, O., 125
Lashley, K. S., 43, 74, 246, 247, 250
Laskiewicz, A., 94
Lassek, A. M., 246
Laszlo, D., 107
Lattin, G. W., 212
Lauffer, P. G., 123
Law, F. E., 36
Lawley, D. N., 412
Lawshe, C. H., 206, 221
Lazarsfeld, P. F., 188, 196, 410, 411
Lazarus, I. P., 228
Lazarus, R. S., 177
Leary, T., 363
Leavitt, H. J., 182
Le Beau, J., 106
Lecznar, W. B., 138
Ledoux, A., 113
Lee, H. W., 2
Leeds, C. H., 16
Leeper, R., 30, 48
Le Gros Clark, W. E., 125
Lehman, C. F., 406
Lehmann, H., 57
Lehner, G. F. J., 294, 312
Leiman, C. J., 303
Leites, N., 190
Leland, E. M., 301
Le Magnen, J., 123, 124
Lennon, R. T., 133, 396
Lennon, V. A., 214
LeQuire, V., 251
Lerner, D., 411
Lersch, P., 163
Levine, L., 324
Levinger, L., 372
Levinson, D. J., 15, 178, 185, 191, 193, 194, 195
Levinson, G., 242
Levitt, E. A., 209, 375
Levy, D. M., 239
Levy, M., 273
Levy, N., 31, 32, 33
Levy, S., 306, 307
Lewin, H. S., 371
Lewin, K., 180, 184
Lewis, D., 95, 406
Lewis, N. D. D., 265
Liberson, W. T., 252
Lichten, W., 75
Lichtenstein, M., 87
Lichtenstein, P. E., 252
Licklider, J. C. R., 95, 97, 99, 100
Liddell, E. G. T., 245
Lidén, G., 94
Lidz, T., 299
Liesch, E., 115
Lindemann, W., 238
Lindner, R. M., 297, 352
Lindquist, E. F., 142
Ling, T. H., 111
Linton, H. B., 36
Lippitt, R., 15, 31, 32, 33, 181, 184
Lipsky, E., 372
Little, K. B., 294, 312
Livingston, R. B., 251
Lockhart, E. E., 119
Loevinger, J., 136
Loewenstein, R. M., 343
Löhrl, H., 239
London, I. D., 30
London, J., 221
Longhurst, J. U., 242
Lord, E., 288, 329
Lorenz, K. Z., 234, 238, 433
Lorge, I., 3, 401
Lott, C. S., 219
Lough, O. M., 373
Low, F. N., 56
Lowenfeld, M., 307, 391
Lowenstein, O., 113
Lubin, A., 146, 413
Luborsky, L. B., 295
Luchins, A. S., 194, 324, 331, 336, 358
Luft, J., 164, 285, 368
Lumsdaine, A. A., 187, 189
Lumsdaine, M. H., 187, 189
Luneberg, R. K., 74
Lurie, S., 75
Luscher, E., 94
Lyerly, S. B., 143, 412
Lynn, J. O., 10

M

Maas, L. O., 227
MacAdam, D. L., 67, 68
McAuliffe, D. R., 86, 87, 101
McCann, W., 324, 334
McCann, W. S., 64
McCary, J. L., 12
McCleary, R. A., 177
McClelland, D. C., 24, 190, 269
McCormick, E. J., 214
MacClure, J. S. R., see Riesco-MacClure
MacCorquodale, K., 29, 33, 170
McCulloch, T. L., 241
McCulloch, W. S., 255, 278
McCouch, G. P., 111
McCurdy, H. G., 167
McDonald, D. A., 251
Mace, C. A., 221
McFann, H., 42
McFie, J., 250
McGhee, M., 388
McGrath, W. B., 266
Macht, M. B., 120
McKee, J. P., 22
McKeown, J. E., 19, 159
Mackworth, N. H., 227, 229
McLeish, J., 171
McLeod, H., 298
McLeod, R. B., 175, 176, 180
McNally, J., 405
McNemar, Q., 160, 308, 413, 414
McQuitty, L. L., 374
McReynolds, P., 57, 289

Magoun, H. W., 256
Maier, N. R. F., 242
Malmo, R. B., 276
Malone, A. J., 24
Malone, F., 77
Malrieu, P., 390
Maltzman, I. M., 32, 36
Manahan, N., 401
Mandell, M. M., 210, 212, 376
Mann, C. W., 118
Mann, I., 55
Mann, J., 332
Manni, E., 116
Manning, G. W., 117
Manor, H. C., 405
Marco, J., 115
Maré, G. de, 87
Marg, E., 56
Margolin, S. G., 273
Marks, E. S., 396
Marks, M. R., 48
Marquis, D. G., 182
Marshall, W. H., 255
Marston, M. V., 238
Martin, G. C., 214
Martin, J. P., 266
Martin, W. E., 14, 194
Martiny, M., 153
Marx, M. H., 44
Maslow, A. P., 214
Mason, E. P., 17
Mason, H. L., 267
Mason, J. M., 215
Massarik, F., 222
Massler, M., 24
Matarazzo, J. D., 310
Matteson, R. W., 360
Mauney, J., 80
May, J., 161
May, R., 171, 435
Mayne, R., 114
Mayo, T. B., 228
Mead, M., 190
Meehl, P. E., 29, 33, 170, 293, 312, 373
Meer, B., 301, 370
Meister, D. E., 177, 178
Mellanby, E., 261, 267
Meltzer, H., 297, 372
Meltzoff, J., 288, 304
Mensh, I. N., 17, 24, 164, 292, 328, 414
Menzer, D., 332
Menzio, P., 116
Mercer, M., 295
Meredith, E. M., 385
Meredith, H. V., 382, 385
Merton, R. K., 188, 189
Meschan, I., 254
Mettler, F. A., 253, 261, 262, 263, 264
Metraux, R. W., 1
Meyer, C. A., 210
Meyer, D. R., 29, 34, 38, 39, 44, 63, 90, 241, 248, 253
Meyer, H. H., 218
Michael, W. B., 139
Michels, W. C., 60, 61, 68
Mickelsen, O., 241, 268
Mickle, W. A., 247
Middleton, J. W., 121, 122
Miles, P. W., 67
Milhorat, A. T., 276
Millard, E. B., 64
Miller, G. A., 50, 85, 95, 100
Miller, H., 6, 324, 335
Miller, H. K., Jr., 228
Miller, J., 366
Miller, J. G., 284, 287
Miller, J. H., 107
Miller, N. E., 3, 24, 30, 36, 244, 322, 333, 344, 345, 352, 354, 355, 357, 419, 422, 429
Miller, S., 431
Milliken, J. R., 162
Milton, E. O., 287
Mirmow, E. L., 4
Mirsky, I. A., 277
Mitchell, H. E., 304
Mitchell, M. B., 210
Mitchell, R. T., 62, 63
Modónski, J., 118
Moericke, V., 243
Molish, H. B., 3, 298
Mollenkopf, W. G., 143, 145, 410, 413
Montgomery, H. C., 87
Montgomery, K. C., 41, 428
Mood, A. M., 415, 416
Moonan, W. J., 402
Moore, H. B., 210
Moore, J. H., 310
Morales, M. F., 117
Moreno, J. L., 347
Morgan, C. L., 30
Morgan, C. T., 91
Morgan, R. L., 45
Morin, F., 256
Morlan, G. K., 125, 193
Morris, A., 62, 63
Morris, R. T., 184, 224
Morris, W. W., 20, 301, 387
Morse, N. C., 187, 189
Morton, R. B., 327
Mosier, C. I., 144, 394
Mote, F. A., 63
Mosteller, F., 412
Mosteller, F. C., 107, 248
Motokawa, K., 73, 250
Mott, J. M., 348
Mowbray, G. H., 75
Mowrer, O. H., 24, 30, 31, 168, 352, 354, 356, 357, 419, 422, 423, 424, 427, 429, 430, 431, 432, 436
Moyer, J. B., 106
Mueller, A. D., 108
Mueller, C. G., 65, 66
Muench, G. A., 286, 301
Muenzinger, F. W., 344
Muenzinger, K. F., 344, 420, 424
Mummery, D. V., 12
Muncie, W., 272, 278, 330
Munn, N. L., 30
Munroe, R. L., 297
Munson, W. A., 88, 92, 110
Murdock, G. P., 11, 190
Murphy, W. F., 331
Müssbichler, A., 242
Mussen, P. H., 14, 196
Myers, C. K., 87
Myers, R. C., 374
Mygind, S. H., 112

N

Nash, M. C., 61
Nathanson, M., 249
Naunton, R. F., 94
Neff, W. D., 249
Neff, W. S., 299
Newcomb, T. M., 175, 187
Newhall, S. M., 61
Newman, H. J., 160
Newton, M., 18
Newton, N. R., 18
Newton, R. L., 308
Nicholas, A. L., 20, 301, 387
Nilsson, G., 94
Nissen, H. W., 30, 48, 240, 241, 249
Noble, C. E., 46
Noirot, C., 240
Noller, P., 5
Noller, P. A., 306, 389
North, A. J., 43, 214
Northrup, G. J., 405
Norton, J. A., 402
Nowlis, V., 22
Nuckols, R., 198

O

Oberndorf, C. P., 343
Obrador, A. A., 266
Obrador, S., 246, 252, 256
Ochs, E., 306
O'Connor, N., 166
Ogden, F. W., 92
Ogle, K. N., 55, 75
Oliver, J. A., 220
Olson, H. F., 89
Orbison, W. D., 373
Orlansky, H., 278
Orringer, D., 273
Orsorio, A., 363
Ort, R. S., 188
Osborne, R. T., 302
Osgood, C. E., 41
O'Shea, H. E., 267
Osterberg, W. H., 224
Otis, J. L., 206
Ovesey, L., 190
Owada, K., 115
Owens, W. A., 211, 376

P

Pace, C. R., 187, 197, 359, 402
Page, H. E., 214
Partridge, M., 261, 264
Pascal, G. R., 305
Passey, G. E., 79, 118
Pastore, N., 161
Paterson, D. G., 226
Patterson, A. V., 401
Patton, R. A., 241, 254
Paul, B. D., 11
Pavlov, I. P., 427
Payne, J. G., 119
Payne, S. L., 196
Peckham, R. H., 78
Peel, E. A., 405
Penfield, W., 106, 109, 110, 116, 120, 246, 252, 255, 261, 264
Pennock, L., 38
Pepinsky, H. B., 352, 358, 362, 365
Perdeck, A. C., 234, 236
Perk, D., 267
Perlman, J. A., 300
Perloff, R., 136
Persky, H., 277
Pestellini, G., 116
Peters, H. M., 242
Petrie, A., 154, 166, 386
Pfaffmann, C., 119, 120
Pfeiffer, J. B., Jr., 274
Phillips, C. G., 245
Phillips, E. L., 12, 19, 189
Pickford, R. W., 56, 69
Piercy, M. F., 250
Pierson, G. A., 353
Pilgrim, F. J., 241
Pilliner, A. E. G., 401
Pincus, G., 271
Pingry, R. E., 405
Pinneau, S. R., 24
Pirenne, M. H., 65
Plaza, L., 69
Polansky, N., 15, 181
Portenier, L. G., 396
Porter, E. H., Jr., 338
Porteus, S. D., 261, 264
Portnoy, B., 311
Poruben, A., Jr., 213, 214
Postman, L., 46, 177, 178, 192, 358
Pottenger, F. M., 18
Pottharst, K., 287
Powell, M. B., 166
Prados, M., 348
Pratt, C. C., 76
Precker, J. A., 306
Prell, D. B., 160
Prentice, W. C. H., 73
Prestwood, A. R., 277
Prevetta, P., 78
Pribram, K. H., 249
Proshansky, H. M., 296

R

Raath, M. J., 155
Rabban, M., 5
Räber, H., 239
Rabin, A. I., 298, 304, 305 308, 310
Rabinowitz, H. S., 48
Radke, M. J., 12, 13
Rafferty, J. A., 144
Raimy, V. C., 328, 330
Ramsey, G. V., 167, 171, 287, 435
Rand, G., 74
Randall, G. C., 335
Randoin, L., 121
Ranke, O. F., 98, 100
Ranzoni, J. H., 300
Rapoport, L., 7
Rappaport, S. R., 310
Raskin, N. J., 328
Rasmussen, T., 106, 109, 110, 116, 120, 255, 261, 264
Ratliff, F., 72
Rautman, A. L., 389
Raven, J. C., 398
Rawnsley, A. I., 92
Redl, F., 15, 181
Reed, H. B., 49
Rees, L., 154
Regelin, C., 373
Regotti, S., 114
Rehberger, J. M., 117, 118
Reichmann, F. F., see Fromm-Rechmann, F.
Reiersol, O., 412
Reiman, M. G., 307
Reyburn, H. A., 155
Reymert, M. L., 437
Ribback, A., 45
Richards, L., 424
Richards, W., 71
Richardson, J., 401
Richter, C. P., 121
Richter, D. E., 261, 263, 268, 275, 277
Rickman, J., 11, 190
Riecker, O. E., 118
Riesch, K. P., 385
Riesco-MacClure, J. S., 86
Riese, W., 250
Riesman, D., 191, 201
Riess, B. F., 304, 372
Riggs, L. A., 71, 72
Rigler, D., 227
Riley, M. W., 16
Riley, R. P., 18
Rimoldi, H. J. A., 140
Riopelle, A. J., 47, 63
Ritchie, B. F., 43
Ritter, A. M., 304, 371
Rittler, M. C., 74
Roberts, E., 305, 372
Roberts, T. D., 113
Roberts, W. H., 425
Robinson, F. P., 352, 354, 365
Robinson, H. A., 404
Robinson, M. F., 264
Roby, T. B., 38, 39, 121
Roe, A., 278, 298, 303
Roeder, W. S., 144
Roens, B. A., 363
Roesler, G., 87
Roff, M., 19, 159, 224, 387
Roff, M. F., 135
Rogers, C. R., 323, 324, 333, 337, 338, 352, 353, 354, 355, 356, 357, 435, 436
Rogers, L. S., 310
Rogers, W. C., 335
Rohrer, J. H., 171
Rokeach, M., 194, 195
Roon, J. M. B. van, see Baerends-van Roon, J. M.
Rosahn, O., 195
Rose, A., 62
Rose, A. S., 107, 248
Rosen, J., 342
Rosenberg, P., 13
Rosenberg, S., 307
Rosenblith, W. A., 85, 86, 90, 92, 93, 96, 97
Rosenthal, S. R., 105
Rosenzweig, M. R., 96, 97, 247, 249
Rosenzweig, S., 4, 164, 200, 286, 306
Roth, M., 250
Roth, W., 43
Rothe, H. F., 210, 211, 213
Rothney, J. W., 363
Rotter, J. B., 289
Rowe, S. N., 106
Royce, J. R., 417
Rubin, A., 117, 118
Rubin, E., 75
Rudolf, G. de M., 332
Ruesch, J., 277
Rulon, P. J., 146
Rundquist, E. A., 218
Russell, D. H., 404
Rutter, D., 405
Ryan, T. A., 77
Rynerson, M. A., 7

S

Sacchi, U., 119
Sachs, E., Jr., 252
Sacks, J. M., 307, 371
Sagarin, E., 121
Sala, O., 114, 117
Saldanha, E. L., 41
Salfield, D. J., 301
Saltz, E., 45
Saltzman, D. C., 62
Saltzman, I., 35
Samter, M., 7
Sanders, W. B., 302
Sanderson, H., 299
Sanford, F. H., 185, 200

XUM

Sanford, R. N., 15, 178, 185, 191, 193, 194, 200
Sarason, S. B., 286, 366
Sata, W. K., 107
Saul, E. V., 41
Sawatsky, J. C., 222
Scarff, J. E., 106, 248
Schachter, S., 180
Schafer, T. H., 88, 89
Schaller, F., 243
Schapiro, H. B., 107
Scheidemann, N. V., 58
Schiff, M., 118
Schiller, P. H., 240
Schlesinger, H. J., 177, 178, 287
Schlesser, G. E., 394
Schlosberg, H., 66, 76
Schmid, J., Jr., 146, 394
Schmidt, R. C., 219
Schneider, K. M., 237
Schneider, L. I., 163, 369
Schofield, W., 329, 369
Schonfeld, W. A., 5
Schreider, E., 154
Schuessler, K. F., 2
Schultz, D. G., 133, 374, 402
Schwartz, A. A., 295
Schwartz, A. M., 107
Schwartz, E. K., 304
Schwartz, L., 171
Scofield, E. H., 122, 124
Scolnik, R., 65
Scott, J. P., 238
Scoville, W. B., 252
Scruggs, J. B., 254
Sears, P. S., 22
Sears, R. R., 22
Seashore, H. G., 375, 398, 399
Seeman, M., 184, 185, 187, 224
Seidman, J., 221
Seitz, A., 238, 239
Seltiz, C., 195
Selye, H., 271
Semmes, J., 74, 250
Sen, A., 154
Sencer, W., 247
Settlage, P. H., 253
Seward, J. P., 29, 31, 32, 33, 290
Sexton, M., 77
Shagass, C., 276
Shapiro, R. B., 214
Share, I., 244
Shartle, C., 186
Shavzin, A. R., 288
Shaw, C. C., 117
Shaw, M. E., 34
Sheatsley, P. B., 197
Sheehan, J. G., 346
Sheffield, F. D., 37, 38, 39, 121
Sheffield, V. F., 42
Sheldon, W. H., 153
Shepard, A. H., 406
Sherif, M., 171, 192, 423
Sherman, M., 10
Sherrington, C. S., 434
Shewmaker, C. A., 89
Shlaer, S., 65
Shoben, E. J., Jr., 352
Shock, N. W., 24
Shostrom, E. L., 364
Siegel, I., 386
Siegel, P. S., 41
Siipola, E., 292, 300
Silverman, S. R., 101
Simmons, L., 11
Simmons, L. W., 180, 190, 275
Simon, C. W., 38
Simon, L. M., 209, 375
Simon, R., 17
Simson, W. A., 16, 388
Sinden, R. H., 68
Singer, J. L., 301, 370
Skard, Ø., 241
Skinner, B. F., 29
Skodak, M., 20
Skouby, A. P., 109
Slater, E., 156, 171, 277
Slavson, S. R., 333
Sleight, R. B., 70, 75
Sloan, L. L., 64
Sloan, N., 251, 255
Slobetz, F., 16, 17
Smith, A. J., 229
Smith, C. J., 214
Smith, F. W., 91
Smith, H. L., 198
Smith, L. C., 403
Smith, M. B., 187, 189
Smith, M. P., 38
Smith, S., 146
Smith, W. K., 252
Smolik, E. A., 106
Snedecor, G. W., 415
Snider, R. S., 256
Snyder, M. M., 391
Snyder, W. U., 312
Snygg, D., 436
Sobel, R., 20
Solomon, H. C., 107, 248, 261, 264, 265, 266
Solomon, R. L., 177
Spache, G., 306
Speakman, T. J., 252
Spearman, C., 394
Speer, G. S., 375
Spence, K. W., 30, 31, 32, 33, 37
Sperling, H. G., 61
Sperry, M., 9, 20, 22
Sperry, R. W., 79
Spiaggia, M., 373
Spitz, R., 1
Springer, D. V., 3, 13
Stacey, C. L., 7, 311
Stagner, R., 170
Standen, J., 162
Standish, C., 332
Star, S. A., 187, 188, 189, 199, 410, 411
Starzl, T. E., 256
Steinberg, J. C., 87
Steiniger, F., 237
Stendler, C. B., 17, 21
Stephens, J. M., 425
Stephenson, W., 164, 165
Sterba, R., 335, 343
Stern, B., 228
Stevens, S. S., 437
Stevenson, J. A. F., 244
Stevenson, W., 361
Stewart, B. M., 146, 375
Stewart, R. G., 200, 374
Stewart, W. A. C., 132
Stewart, W. B., 111
Stiles, F. S., 10
Stockford, L. O., 221
Stoke, S. M., 24
Stoll, W., 107
Stone, A., 324
Stone, C. H., 226
Stone, C. P., 24
Stone, G. R., 10, 425, 426
Stouffer, S. A., 187, 188, 189, 199, 410, 411
Strang, R., 328, 363, 403
Strange, J. R., 34
Strassburger, R. C., 34
Strauss, A., 2
Strauss, A. L., 2
Strong, E. K., Jr., 132, 209, 374
Stuit, D. B., 365
Stuntz, S. E., 89
Succi, G., 179
Suchman, E. A., 187, 188, 189, 199, 410, 411
Sullivan, H. S., 323, 423
Summerfield, A., 166
Sumner, F. C., 71
Super, D. E., 208
Sutherland, J., 13, 139, 405
Suttell, B. J., 305
Sweet, W. H., 254
Symonds, P. M., 7
Szabo, T., 116
Szentágothai, J., 116
Szmanski, M. G., see Gavrel-Szmanski, M.

T

Talbot, S. A., 247
Tannenbaum, R., 222
Tanner, W. P., Jr., 67
Taub, N., 261
Taubman, R. E., 95
Taylor, C. W., 144
Taylor, E. K., 143, 217, 218
Taylor, F. K., 336
Taylor, H. L., 241, 268
Taylor, J. A., 34
Taylor, J. G., 29

Taylor, V., 292, 300
Taylor, W. G., 100
Teel, K., 35
Temmer, H. W., 37
Tenen, C., 14, 193
Terry, D., 293, 371
Teuber, H., 248
Thauer, R., 109
Thetford, W. N., 3, 298
Thibaut, J., 180
Thibaut, P., 123
Thiesen, W. G., 298
Thistlethwaite, D., 31, 32, 33, 169, 178
Thomas, E. B., 64
Thomas, R. W., 303
Thompson, C. E., 304
Thompson, C. W., 331, 353
Thompson, G., 14
Thompson, G. G., 3, 15, 17
Thompson, G. M., 299
Thompson, I. M., 248
Thompson, J. M., 247
Thompson, P. O., 89
Thompson, W. C., 252
Thomson, A. F., 252
Thomson, L. C., 59
Thorndike, E. L., 419, 425
Thorndike, R. L., 145, 211
Thorne, E., 339
Thorne, F. C., 352, 353, 354, 355, 356, 357, 365
Thorpe, W. H., 234, 236, 240
Thune, L. E., 46
Thurstone, L. L., 80, 138, 142, 209
Tiedeman, D. V., 146
Tiffin, J., 215, 216, 226
Tilton, J. W., 425
Timm, C., 243
Timm, O. K., 348
Tinbergen, N., 234, 235, 236
Tinker, M. A., 404
Tizard, J., 166
Toby, J., 188, 199
Tolman, R. S., 284
Topal, J. R., 228
Torrance, P., 346
Towbin, E., 244
Townsend, E. A., 2
Tousey, R., 65
Trager, H. G., 12, 13
Trankell, A., 390
Travers, R. M. C., 397
Travers, R. M. W., 3, 142, 375
Troland, L. T., 420
Troyer, M. E., 402
Tsao, J. C., 47
Tucker, J., 147
Tucker, J. E., 301, 369
Tunturi, A. R., 98
Tupes, E. C., 207, 285, 367
Tyler, D. B., 117

U

Uhrbrock, R. S., 219
Umberger, J. P., 309
Umrath, K., 242
Underwood, B. J., 44, 45, 46
Urmston, R., 241

V

Valentine, C. W., 393
van Deinse, J. B., see Deinse, J. B. van
van der Horst, L., see Horst, L. van der
van der Velden, H. A., see Velden, H. A. van der
Van Egmond, A. A. J., 116
Van Eyck, M., 113
van Roon, J. M. B., see Baerends-van Roon, J.M.
Velden, H. A. van der, 66
Verheijen, F. J., 244
Vernon, P. E., 210, 212, 395, 404
Viaud, G., 243
Vignes, H., 108
Vilstrup, T., 112, 116
Vinacke, W. E., 48
Voeks, V. W., 30
Volkman, J., 189
von Allesch, J., see Allesch, J. von
von Aschan, G., see Aschan, G. von
von Bertalanffy, L., see Bertalanffy, L. von
von Bonin, G., see Bonin, G. von
von Frisch, K., see Frisch, K. von
von Holst, E., see Holst, E. von
VonLackum, W. J., 300
Votaw, D. F., Jr., 144

W

Wade, M., 43
Waelsch, H., 267
Wagner, R. F., 142, 215
Wald, G., 66
Waldfogel, S., 310
Walker, A. E., 252
Walker, K. F., 391
Wall, W. D., 16, 388
Wallace, W. L., 375
Wallach, H., 171
Wallin, P., 18
Walter, W. G., 251
Waltner, J. G., 112
Walton, R. E., 306
Walzl, E. M., 97
Wang, S. C., 117
Wapner, S., 79
Warburtin, F. W., 135
Ward, A. A., 115
Ward, J. W., 251
Ward, L. C., 285, 371
Ward, W. D., 90, 93
Warkentin, J., 324, 347
Warner, S. J., 310
Warren, J. M., 90
Warren, M., 68
Warren, W., 157
Warson, S., 20
Warwick, R. T. T., 247
Waters, R. H., 34
Watson, J. B., 425
Watson, R. I., 213, 328
Watts, J. W., 261, 263
Wauck, L. A., 312
Weale, R., 64, 70
Webb, W. B., 35, 310
Webster, E. C., 220
Webster, J. C., 87, 95
Wechsler, D., 384
Weeks, E. J., 386
Wegel, R. L., 89
Weider, A., 5, 296, 306, 389
Weinstein, E. A., 250
Weise, P., 30
Weisgerber, C. A., 373
Weisgerber, S. J., 373
Welch, K., 246
Welford, A. T., 228
Wendt, G. R., 112, 113, 115
Wenzel, B. M., 121, 122
Werner, H., 2
Werner, S., 374
Wertham, F., 307
Weschler, I. R., 200, 224
Wesley, S. M., 146, 375
Wesman, A., 398
Wesman, A. G., 145
West, E. D., 209
Westcott, H. F., 124
Westie, C. M., 183, 187
Wever, E. G., 99, 100
Wexler, M., 302, 306
Wheeler, D. E., 91
Wheeler, J. I., Jr., 310
Wheeler, W. M., 294, 312
Wherry, R. J., 136, 144, 218
Whitaker, C. A., 324, 347
White, R. K., 184
Whiting, B. B., 11
Whiting, J. W. M., 11, 190, 427
Whitty, C. W. M., 251
Wickens, D. D., 38
Wiener, D. N., 373, 374
Wiener, F. M., 88
Wilcox, P., 347

XUM

Wilkins, W. L., 310
Wilkinson, B., 414
Wilks, S. S., 415
Willgoose, C. E., 388
Williams, E. M., 406
Williams, K. A., 429
Williams, M., 171, 250, 252, 254, 256, 292
Williams, R. D., 267
Williams, R. M., Jr., 187, 189
Williamson, E. G., 352, 358, 363
Wilson, G. P., Jr., 302
Wilson, W. H., 92
Wiltshire, H. M., 307
Winch, R. F., 20
Windle, C., 262
Wing, K. G., 85
Winklestein, A., 273
Winn, A., 220
Winne, J. F., 312, 373
Winsor, A. L., 405
Winstanley, N. B., 214
Winston, J., 117, 118
Wischner, G. J., 424
Witkin, H. A., 79, 80
Witryol, S. L., 3, 14, 17
Witt, P. N., 242
Wittenborn, J. R., 164, 284, 290, 294, 302, 303, 370, 371, 413
Wittson, C. L., 286
Wohlfahrt, T. A., 244
Wolf, S., 271, 273, 275
Wolfenstein, M., 190
Wolff, D., 242
Wolff, H. G., 105, 106, 248, 271, 272, 274
Wolff, W. M., 214
Woltmann, A. G., 305
Wood, A., 424
Wood, A. B., 339
Wood, A. W. S., 24
Wood, C. L., 77
Woodbury, C. B., 49
Woolsey, C. N., 97, 247, 248, 249
Worbois, G. M., 134, 147, 218
Worchel, P., 80, 115, 254
Wormser, M. H., 195
Wortis, J., 171
Wrenn, C. G., 352, 354, 365
Wright, E. D., 70
Wright, G. H., 108
Wright, H. F., 21, 22, 180
Wright, M. R., 109
Wright, S. C., 295
Wulff, J. J., 39
Wylie, R. C., 47

Y

Yakovlev, P. I., 254
Young, J. Z., 252
Young, N., 17
Young, R. A., 330

Z

Zabarenko, L. M., 241
Zachert, V., 138
Zangemeister, H. E., 94
Zangwill, O. L., 250
Zeaman, D., 35
Zeaman, J., 271
Zeligs, R., 14
Zimmerman, W. S., 139, 376
Zobel, S. P., 226
Zotterman, V., 120
Zubin, J., 171, 253, 262, 264, 271, 290, 291, 293
Zur Neiden, M., 160, 386
Zwislocki, J., 94, 98

SUBJECT INDEX

A

Abilities
of children, 384-85
levels of, 395
listening, 397
mathematical, 405
mental
honor awards and, 396
psychosurgery on, 264-65
pupil-migrancy on, 401
reading, 398, 401-4
in visual thinking, 80
see also Intelligence
Achievement testing, 399-406
individual differences and, 133
intelligence level and, 396
motivational factor and, 402
musical, 405-6
pupil-migrancy and, 401
scholastic prediction by, 399-400
vocabulary, 404-5
see also Tests
Adolescence
blindness in, frustrations of, 8
physical maturation in, 4, 384-85
physical sex characteristics in, response to, 5-6
psychosexual development in, 5-6
adult interest in, 6
psychotherapy and, 6
somatic androgyny ratings of, 5
Animal psychology
auditory experiments in, 243-44
autonomic reactivity, frontal lobes in, 252
behavior inventories in, 238
bilateral leucotomy in, 253
color vision studies in, 243
diencephalic centers and, stimulation of, 251
emotion in, frontal lobes on, 252
gastric stimuli and, 244
hemidecortication in, 249, 253
intelligence in, brain lesions on, 253
latent learning and, 31-34
oral-pharyngeal stimuli and, 244
peripheral sensitivity and androgens on, 242
estrogens on, 242
predatory behavior and, 239
prefrontal lobotomy in, 253
social organization in, 237-38
dominance ranking in, 237-38
herding in, 237-38
somesthetic stimuli in, differential reaction to, 249
stimulus-response in, peripheral mechanisms of, 243-45
thalamic retrograde degeneration and, 249
visual stimuli in, differential reaction to, 249
see also Comparative psychology; and Sensory processes
Anxiety
ego psychology and, 435-37
fear and, 436
on learning, 34-35, 168
motivation and, 423-24, 435-37
neurotic, 354, 436
normal, 354, 436
personality and, 436
psychosurgery on, 262-63
Appetite, secondary reinforcement and, 428-32
Aptitude testing
fine arts, 376
individual differences in, 137
intellectual, 375-76
intelligence testing and, 394-99
occupational, 397
special, 375-76
see also Tests
Attitudes
behavior and, 187-89
cognitive distortion by, 178-79
error-choice measurement of, 200
factor analysis of, 199-200
Guttman's scale analysis of, 199
indirect measurement of, 200-1
latent-structure approach to, 199
measurement of, 374-75
methodology of, 196-201
projective measurement of, 200-1
public opinion and, 196-201
scale for measuring, 388
social framework of, 187-89
tests of, 200-1
unidimensional measurement of, 198-200
see also Opinion methodology
Audition, see hearing

B

Behavior
attitudes and, 187-89
autonomic components of, 251-52
avitaminosis on, 267
biochemistry and, 266-67
chemical factors in, 241-42
culture on, 357-58
diet on, 241-42
drives on, 420
secondary, 421-24
drugs on, 242
emotion-blood relationships and, 276-77
external stimulus on, 421
fear on, 421-22
flexibility on intelligence, 396
group
of children, 15
contagion on, 181-82
limited communication on, 182
hormones on, 242
instinctive, 236-40
analysis of, 234-36
birds, 238-39
fish, 239
insects, 239-40
mammals, 236-38
sexual behavior patterns and, 236-37
internal stimulus on, 421
inventories of, 238
leucotomy on, 253-54
noise on, 85
organzational 183-87
phenomenological approach to, 357-58
reactive inhibition on, 41

XUM

relativistic concept of, 357
role conflict on, 188
semistarvation on, 268-71
social, 176-80
accurate social perception on, 179-80
cognitive processes in determinants of, 177-78
distortion of, 179
environmental settings for, 180
ethnocentric attitudes on, 178
framework of, 187-89
perception in, 177-80
warm-cold variable, 179
see also Group dynamics; and Perception, social
verbal exchanges on, 359-60
see also Behavior abnormalities; Behavior of children; and Personality
Behavior abnormalities, 157-58, 261-79
biochemical tests and, 267
brain on
alteration of, 261-66
metabolism of, 267
classifications of, 366
emotional variables and, 387
genetic factors in, 277-79
internal environment on, alterations in, 266-71
intrafamily resemblances in, 19-20
life-stress on, 271-77
maladjustment and, sources of, 354-55
memory loss and, 271
neurosis as learning deficit, 353-54, 357
personality tests and, 386-87
predisposition for, 278
psychosurgery on, 261-66
Rh factor on, 279
Self-regarding Span Test and, 264
Sensibility Questionnaire and, 264
shock therapy on, 271
threats on
physical, 272
symbolic, 272
twin study of, 278
see also Behavior; Behavior of children; and Delinquency
Behavior of children
age on, 1-6
allergy on, 6-7
breast feeding on, 18
class room influence on, 16-18
environmental factors on, 21-22
experimental modification of, 8-11
family attitude on, handicaps and, 7-8
family situation on, 18-21
in adoptive homes, 20
punishment and, 20
feeding on, 9
frustration on, 10
handicaps on, 6-8
illness on, 6-8
kinesthetic experiences on, 9
mass communications on, 15-16
maternal attitudes on, 157-58
parental behavior on, 159
physical maturation on, 4
progressive education on, 17
self-concepts and, false reports on, 9-10
sex on, 1-6
sociocultural factors on, 11-21
teacher attitude and, 16-17
training on, 20-21
verbal
modifiability of, 10
social pressure on, 10-11
weaning on, 18, 159
see also Behavior; Child psychology; and Children
Bender Visual Motor Gestalt test, 296, 305-6
Brain
biochemistry of, 266-71
cerebral interrelationships in, 252-56
cerebral metabolic rate, 267-68
damage on, 252-54
diencephalon in, role of, 251
frontal lobe in, psychological function of, 262, 264
function, alteration of, 261-66
integrative mechanisms of, 254-56
intracortical connections in, 255
parietal cortex role in, 266
reticular system in, 255-56
structure
alteration of, 261-66
see also Psychosurgery

C

Chemical sense, 119-25
see also Olfaction; and Taste
Child psychology, 1-24
behavior experiments in, 8-11
developmental studies in, 3-4
sociocultural factors in, 23
ethnic factors in, 14-15
ethnological factors in, 11-12
group behavior and, 15
learning theory and, 22-23
racial factors in, 12-14
regional factors in, 12
reinforcement theory and, 8-9
research theories in, 21-24
sex difference studies in, 4-6
social-class factors in, 12
training practices, history of, 21
transposition studies in, 8-9
incentives and, 9
see also Behavior of Children; and Children
Children
abilities of, 2, 384-85
adopted
environment on, 20, 386
follow-up of, 386
backward
education of, 392,93
practical aptitude and, 394
breast feeding of
behavioral effects of, 18
maternal attitude toward, 18-19
physical effects of, 18
concept formation in, 2-3
conduct disorders in, 157-58
epilepsy on, 158
psychopathy on, 158
copying ability of, 2
disturbed
behavioral contagion on, 181
family situation on, 19-20
drawing in, age on, 390
egocentric responses in, 389
emotional development in, 3-4
group differences in, 134-35
handicapped
frustration of, 8

social reactions of, 390
illness experience of, 6-8, 391-92
intelligence of, bilingualism on, 132
learning, in, 8-11
left-handedness in, 390-91
cerebral palsy and, 391
heredity of, 391
masculinity of, 5-6
motion pictures on, 388
motor responses of, 1
neurotic disorders in, 157-58
prejudice in, family influence on, 14-15
psychoanalysis of, 342-43
reading ability of, 2, 398, 401-4
Rorshach test on, 298
school readiness of, 392
sex awareness of, 5-6
socio-economic level on, 389
sex differences of, 4-6, 15-16
cultural background on, 4-5
and somatic androgyny, 5
social value discrimination in, 17
somatic-psychic interrelationships in, 382-85
speech development of, 1
suggestibility in, 389
weaning of, behavioral effects of, 18, 159
see also Behavior of Children; Child Psychology; and Personality, development of
Cognition
and cerebral interrelationships, 252-56
convulsive shock on, 254
distortion in, 178-79
ethnocentric attitudes on, 178
learning theory and, 33
social factors in, 178
see also Learning; and Perception
Color blindness, 67-68
causes of, 69
color discrimination and, 69
deviants in, 69
luminosity function on, 69
measurements of, 69
see also Vision, color
Communication
group, 180-82
in industry, 225-26
mass, on children, 15-16
psychotherapy and, 326
Comparative psychology, 223-56
autonomic activity, cortical influence on, 251-52
behavior, chemical factors in, 241-42
displacement activity, 236
emotion, frontal lobes on, 252
evolutionary origins and, 233-34
gastric motility, 252
cingulate regions on, 252
orbital regions on, 252
hypothalamic parasympathetic centers, 252
innate behavior patterns 234-36
endogenous movements and, 234, 236
hierarchichal nervous organization of, 235
phyletic homologies of, 235
integrative mechanisms, 254-56
intention movements, 235-36
interspecies relations, 235
intraspecies communication, 235-36
mass facilitation, 255
naturalistic approach to, 233-34
perception and, 240-41
phylogenetic studies
development, 233-34
innate behavior, 236-40
learning, 240-41
perception, 240-41
social releaser concept in, 235
status of, 233-34
stimulus situation study, 236
see also Animal Psychology; Learning; and Physiological Psychology
Counseling, 351-76
adjustment ratings in, 359
basic issues in, 353-54
client-centered approach in, 364
client-counselor responsibility in, 360
client-participation in, 359-60
definitions of, 352
"depth psychotherapy" and, 352
diagnosis and, 365-76
ethical principles for, 353
evaluation of, 363-64
control groups in, 363-64
limitations of, 363
problems of, 363
experimental psychology and, 351
group, 362-63
interpersonal influence in, 181-82
learning and, 351
methodological possibilities in, 351-52
normal anxiety and, 436
process, analysis of, 358-62
professional status of, 352-53
psychotherapy, nondirective, and, 351
self-conceptualization changes from, 359
social responsibilities of, 353
theories of, 353-58
therapeutic relationship in
analysis of, 358-62
communication in, 362
emotional distance in, 362
expert-novice studies of, 361-62
as function of expertness, 361, 362
ideal, concept of, 360-61
pooled ratings of, 361
quantifying, 360-62
therapist in, status of, 362
therapy, 332-36, 352-64
training in, 353
multiple therapy in, 353
transference in, 356-57
trends in, 352-53
verbal exchanges in, 359-60
see also Group psychotherapy; and Psychotherapy

D

Delinquency, 155-56
body build on, 156
Rorshach test and, 155
Diagnosis
attitude measures and, 374-75
classifications of, 365-66
counseling and, 365-76
devices in, 366-69
ethical standards for, 367
importance of, 366
intellectual aptitude test and, 375-76
interest measures and, 374-75
predictions of success and, 367

problems of, 365-66
projective procedures in, 369-74
special aptitude tests and, 375-76
see also Psychodiagnostics; and Tests
Drive
appetite as, 428-32
anxiety as, 423-24
beneceptive, 420
increment, 420-24
learning and, 34-36
nociceptive, 420
reduction, 420-24
by secondary reinforcing agents, 38
organism-making-response and, 420
reinforcement and, 345-46
Yale theory of, 290
see also Motivation theory; Reinforcement; and Stimulus-response sequence

E

Ear
cochlea in
electrical stimulation on, 113-14
electrophysiology of, 86-87
summating potential in, 87
cochlear microphonic in, body temperature on, 86
inner, receptor action in, 113
neural action potential and, body temperature on, 86
see also Hearing; and Vestibular functions
Education
backward children and, 392-93
practical aptitude of, 394
character development and, 381-82
cheating and, 405
cultural patterns on, 399
cumulative record-making in, 386
curriculum improvement in, 400-1
emotional life and, 381-82
evaluating outcomes of, 402
habit formation and, 381-82
individual differences and, 381
instructional supplements in, 402
moral aspects of, 399
organization of environment in, 400-1
poetry discrimination and, 404
progressive, on children, 17
psychological methods and, 393
pupil-migrancy in, 401
reading determinants in, 402
reading improvement and, 403
selection for secondary school in, 397-98
see also Adolescence; Child psychology: Educational psychology; Learning; and Reading
Educational psychology, 381-406
achievement tests and, 399-406
aptitude testing and, 399-406
body build and, 382-84
child development and, 381, 382-94
environment on, 386-87
factorial methods and, 395
intellectual efficiency and, nonverbal test of, 398
intelligence testing and, 394-99
learning process and, 381-82
musicians and, background factors of, 406
practical aptitude and, 393-94
reading difficulties and, 402-3
school learning and, 399-406
somatic-psychic interrelationships and, 382-85
speech difficulties and, 399-406
study-methods and, 400
teacher and, mental life of, 381
test ratings in, honor awards and, 396
theoretical aptitude and, 393-94
Wisconsin Adaptation M-Blank and, 385
see also Adolescence; Child psychology; Education; and Learning; and Reading
Ego psychology
anxiety and, 435-37
ego strength and, 284-85
motivation theory and, 423-24, 435-37
Extinction, 42-43
autistic satisfaction and, 431
conditions of, 42-43
on learning, 42-43
massed training on, 42
nervous structure and, 249-50
reinforcement, partial, and, 36-37
spaced training on, 42

F

Factor analysis, 137-41, 412-13
applications of, 140
of attitudes, 199-200
correlation coefficients and, 138-39
of educational attainments, 395
experimental methods in, 140-41
of individual differences, 137
job evaluation, 216
latent structure analysis and, 411
of mechanical abilities, 209-10
mental measurement by, 395
of organizational behavior, 187
orthogonal versus oblique rotations and, 139-40
of personality, 164-66
psychodiagnostics and, 294-95
P-technique of, 295
Q-technique of, 165-66
residuals in, 412-13
of Rorschach test, 294
rotation in, 138-40
second-order factors on, 140
selection testing and, 208
statistical methods and, 412-13
see also Tests
Fear
anxiety and, 436
conflict theory and, 422
definitions of, 435-36
drive onset on, 428
drive termination on, 428
external stimulus on, 421
in handicapped children, 7
internal stimulus on, 421
as secondary drive, 421-22, 425
see also Anxiety

G

Gestalt personality theory, 289-90
Group dynamics, 180-83
  communication in, 180-82
  conference process, 182-83
  dimensions of, 183
  experimental variables in, 180-81
  reference group theory in, 188-89
  social role in, 188
  see also Leadership
Group psychotherapy, 332-36, 352-64
  applications of, 363
  atmosphere in, permissive, 336
  diary-keeping in, 334
  effectiveness of, 321
  epileptics in, 335-36
  film therapy in, 348
  homogeneity in, 336
  individual therapy and, 333
  patient as therapist in, 333-34, 334-35, 363
  prejudice and, 195-96
  psychotics and, 333, 334-35
  recording devices in, 324
  self-concepts and, changes in, 333
  self-revelations in, 336
  sexual material from, 335
  social techniques from, 333
  therapists in
    participation of, 324, 331
    training of, 336
  transference interpretations in, 335
  see also Counseling; and Psychotherapy

H

Hearing, 80-102
  absolute threshold measurements in, 87-88
  adaptation deafness and, 92
  audiometry, 87-88, 385-86
    galvanic skin response in, 88
  auditory cues in, 96
  auditory fatigue in, 91-92
    measurements of, 92-93
    in neural system, 92-93
    perstimulatory, 91-92
    poststimulatory, 91-92
    recovery from, 92
    short duration, 92
    susceptibility to, 91-92
  best-beat technique and, 90-91
  binaural beats in, 95-96
  cortical functions in, 248-49
  information theory in, 85
  loss of
    critical intensity for, 91
    diagnostic criteria for, 93
    stimulation on, 91-92
  loudness recruitment in, 93-95
    absolute threshold variability in, 94
    measuring techniques for, 93-94
  loudness scale, 88-91
  masking in, 87-90
    critical band in, 89
    pitch and, 90
  nerve deafness and, 91-92
  nervous system in
    binaural interaction in, 96-97
    frequency selectivity of, 97-98
  neural action potential in, 96-97
  noise on, 85
  perceptive deafness and, 91-92
  pitch in, 95-96
    duplex theory of, 100
    intermittency on, 100-2
    trill thresholds in, 95
  pitch discrimination in, 98-102
    beam theory of, 99
    curvature theory of, 99
    eddy theory of, 99
    gradient theory of, 99
    neural theories of, 99-100
  psychological factors in, 227-28
  reading difficulties and, 402-3
  sound localization in, 96
  speech intelligibility and, 97
    amplitude distortion on, 97
    binaural hearing aids on, 97
  stimulation deafness, 92
  theories of, 98-102
  vibrato, preference for, 95
  see also Ear; and Sensory processes
Hyperalgesia, 105-6
  induced, 105, 106
  natural, 105
  see also Pain

I

Individual differences, 131-47
  age on, 133
  color vision, 56
  education and, 381
  environment on, 131-35, 160-61
  factor analysis of, 137
  heredity on, 131-35, 160-61
  innate capacities, measurement of, 131-32
  nationality and, 134-35
  parent-child resemblances, 135
  physique, 141
  race, 134-35
  sex, 134
  stability of, 132-33
  talent range of, 144-45
  temperament, 141
  traits, 135-37
    homogeneity of, 135-36
    independence of, 136-37
    stability of, 132-33, 137
    type versus, 141-42, 152
    varieties of, 137
  types, 141-42
    Sheldon, 141
    traits versus, 141-42, 152
  see also Personality
Industrial Psychology, 205-30
  accidents and, 229-30
  communication and, 225-26
  criteria of job success, 217-20
    biasing factors, 217-18
    evaluation of, 218
    ratings, 218-19
    value judgments on, 217
  Critical Incidents Technique, 209-10
  decision-making in, 222-23
  exchange of ideas in, 206
  group interviewing and, 210
  group relations and, 222-26
  human engineering and, 226-29
    fatigue, 227-28
    perceptual-motor functions, 227-28
    physiological conditions, 229
    reaction times, 228
  incentive systems and, 221
  industrial psychologist and, 205-6

XUM

interviewing in, 206-8
tests and, 207-8
training in, 206-7
validity of, 207-8
job turnover and, 222
labor-management relations and, 223-24
labor mediators, 224
leadership and, 224-25
merit rating in, 219-20
morale, 221-22
occupational level, intelligence and, 209
participation, 222-23
perceptual-motor functions and, 227-28
performance rating in, guide to, 218
placement testing in, 208-15
problems in, 211
promotion potentiality, 212-13
ratings, 218-20
readability formulas, 225-26
selection testing, 208-15
administrators, 212
clerical, 213
professional personnel, 211-13
protective service employees, 213-14
semiskilled workers, 214-15
skilled workers, 214-15
supervisory personnel, 211-13
tension levels in, 228
training in, 220
turnover, 222
see also Job analysis; Job evaluation; and Tests
Intelligence
behavior flexibility on, 396
cultural-educational level and, 398
cultural patterns and, 399
deterioration, age on, 311-12
environment on, 160-61
expressional level and, 401
glutamic acid on, 162, 241-42
group testing of, 398-99
heredity on, 160-61
occupational level and, 209
practical aptitude and, 393-94
psychosurgery on, 265
sex on, 383
testing of, 394-99
aptitude testing and, 394-99
Interviewing, see Industrial Psychology
IQ
constancy of, 394
performance, 375, 399
verbal, 375, 399

J

Job analysis, 215-17
Critical Incidents Technique in, 215
sampling activity in, 215
test development and, 215
see also Industrial psychology; and Job evaluation
Job evaluation, 215-17
fallacies in, 216
systems of, 216-17
variables in, 216
see also Industrial psychology; and Job analysis

K

Kuder Preference Record, 5, 212, 213, 214, 383
faking scores and, 211

L

Latent learning, 31-34
anticipatory goal responses and, 32
cognition theories and, 31, 33
drive externalization in, 33
high motivation in, 33-34
irrelevant-incentive and, 31-33
motivation theory and, 432-33
perceptual responses and, 33
reinforcement theory and, 31, 32-33
secondary drives in, 168-69
see also Learning
Leadership, 183-87
authoritarianism, 185-86
definitions of, 184
democratic, 184-87
equalitarianism, 185-86
factors on, 184-85
group attitudes on, 185
group interview tests for, 210
group size on, 185
in industry, 224-25
leader-follower relations, 186-87
organizational behavior and, 183-87
organizational studies of, 186-87
styles of, 185
variables in, 184
Learning, 29-50
of adequate social techniques, 333
annoyers on, 425-26
anxiety on, 34-35, 168
children, 8-11
concept formation in, 49
in children, 2-3, 48
simultaneous versus serial presentation in, 49
conditioning in, 168
continuity in, 43-44
counseling and, 351
cues in, 43, 48
discrimination, 38, 43-44
theory of, 29-30, 44
transposition in, 45
discrimination-reversal, 43-44
theory of, 44
variables in, 43
drive state in, 34-36
conditioning and, 34-35
extinction and, 34-35
habit strength and, 34-35
zero, 35
external stimulus on, 421
extinction in, 42-43
massed training on, 42
spaced training on, 42
generalization in, 44-45
gradients of, 44-45
measurement technique for, 45
grades of, 236
incentive variables in, 38-40
incidental, 31-33
insight, 48
internal stimulus on, 421
language
autistic satisfaction in, 430-31
secondary reinforcement in, 430-31
manipulation drive in, 39
"means" from, 423
methodological study in, 50
motivation and, 419-20
motor skills, 46-47
discrimination transfer in, 46-47
massed practice in, 47
spaced practice in, 47
predifferentiation in, 47
retention in, 46-47
multiple-alternation problems in, 49-50
perception and, 240-41
perceptual factors in, 40-41
problem-solving, 168, 431
aids in, 48-49
psychotherapy and, 344-46
punishment on, 420, 424-28

reactive inhibition and, 41-42
reasoning and, 48-50
reinforcement and, 8-11, 34-40
gradient of, 428
research trends in, 50
reward on, 38-40, 420, 424
in school, 399-406
sign, 423, 432-33
emotions from, 423
"meanings" from, 423
solution, 423, 432-33
instrumental acts from, 423
spread of effect in, 44
stuttering as product of, 346
temporal factors in, 40
extinction and, 40
theories of, 29-30, 168-70
thinking and, 48-50
transfer effect in, 47
transposition in, 44-45
gradients of, 45
two-factor theory of, 431-32
verbal, 45-46
anticipation in, 45
recall in, 46
relearning in, 46
reminiscence on, 46
serial position curve in, 45-46
warm-up trials in, 46
see also Education; Educational psychology; Latent learning; Perception; Psychotherapy; and Reinforcement

M

Maturation
growth process and, 384
increase in, rates of, 384
measurement of, 372
physical, in adolescents, 4, 384-85
process of, 383
puberty, 384
sexual, 5-6, 384
social responses to, 4, 385
see also Maturity
Maturity
concept of, 383
expressiveness and, 385
increase in, rates of, 384
indicator of, 384
morphological changes and, 383-84
physical attractiveness and, 385
puberty as measure of, 384
sexual maturation and, 384
see also Maturation
Memory, see Retention
Mental defectives, social reactions on, 7-8
Mental measurements, 394-99
factorial methods in, 395
rational-hypothesis approach to, 397
statistical aspects of, 409-12
technicians's approach to, 397
theoretical concepts in, 394-95
see also Statistical methods; and Tests
Minnesota Multiphasic Personality Inventory, 34-35, 312, 373-74
coding of, 293
configural scoring, 293
diagnostic groups, differentiation of, 312
factor analysis of, 294-95
interpretation of, 373
psychotherapy outcomes and, 329
related tests and, 373-74
Morale, 221-22
factor analysis of, 187
grievances on, 221
job satisfaction and, 221-22
motivation and, 221-22
salary on, 221
Motivation, 221-22
anxiety as, 423-24, 435-37
frequent testing as, 402
morale and, 221-22
salary as, 221
tertiary, 29
see also Drive; Learning; and Motivation theory
Motivation theory, 419-37
appetite drives and, 428-32
ego psychology and, 423-24, 435-37
hedonism and, 419
historical development of, 419-24
latent learning and, 31-34, 432-33
learning and, 419-20
mechanistic schools of, 433-34
punishment role in, 424-28
secondary motivation, concept of, 423
secondary reinforcement and, 37-38, 428-32
servo mechanisms and, 433-35
teleology and, 433-35
see also Motivation; and Stimulus-response sequence

N

Nervous system
binaural interaction in, 96-97
defects of, reading and, 402-3
frequency selectivity in, 97-98
instinctive behavior, hierarchical organization for, 235
olfaction, mechanisms in, 124-25
personality and, 170-71
reasoning levels and, 30
sensory processes and, 246-51
Nystagmus, see Vestibular functions, nystagmus and

O

Olfaction, 121-25
Cornell Olfactorium, 122
individual differences in, 121
molecule-enzyme alliance in, 123
neural mechanisms in, 124-25
odor classification, 124
odor discrimination in
age on, 125
measurement of, 123
molecular configuration on, 123-24
olfactory bulb in
anesthesia on, 125
electrical activity of, 124-25
epithelium on, 125
peripheral mechanisms of, 122-24
scientific standards in, 121-22
sensitivity in, body chemistry on, 124
theories of, 122-23
molecular configuration, 122-23
see also Chemical taste
Opinion methodology, 196-200
bias
in interviewers, 197-98
in wording, 199
interviewing, 197-98
National Opinion Research Center, 197-98
projective methods, 200

XUM

undimensional measurement, 198-200
validity of, 197
see also Attitudes

P

Pain, 105-8
central mechanisms of, 106
cortical representations of, 106
cutaneous, 105-6
histamine on, 105
drugs on, 107
gonadal, 106
intractable root, 108
natural, measurement of, 107-8
peripheral mechanisms of, 105-6
prefrontal lobotomy on, 106-7
quantification of, 107-8
spinothalamic tractotomy on, 108
stress, 108
threshold determinations in, 106-7
uterine, 108
visceral, 108
see also Hyperalgesia; and Somesthesis
Perception
constancy in, 170
extrasensory, 332
individual differences, 177
learning and, 40-41, 240-41
personality and, 166-68
social, 178-79
accuracy in, 179-80
bias in, 179
distortion in, 179
see also Behavior, social
social determinants of, 177-78
see also Learning
Personality, 151-71
anxiety on, 423-24, 436
autonomic functioning in, 153
basic structure of, 190
body build and, 141, 151-52, 153, 154, 156
delinquents and, 156
clinical students, assessment of, 162-63
culture and, 189-91
development of, 156-61
differential, causes for, 157-58
environment on, 156-58, 160-61
epilepsy on, 158
family patterns on, 20
heredity on, 156-58, 160-61
heredity-environment interaction on, 158
oral gratification factor on, 158-59
parental behavior on, 157-58
psychoanalytic assumptions of, 190-91
pyschopathy, 158
weaning on, 159
dimensions of, 154
ego constancy and, 170
embryonic constitution on, 153
end point of, 436-37
experimental concepts of, 169-70
experimental testing of, 166-71
extroversion-introversion in, 154-55
handicaps on, 7-8
homeostatic concept on, 170
hypnosis on, 346-47
infant experience on, 278-79
integration in, 423
intelligence tests and, 154
intrafamily resemblances in, 19-20, 159-60, 387-88
juvenile delinquency and, 155-56
Kretschmer, 151-53
learning and, 168
measurement of, 161-66
criterion analysis in, 166
factorial techniques in, 164-65
"Q" technique in, 165-66
statistical methods in, 164
methodology of, 161-66
nationality on, 189-90
neurological systems and; 170-71
neurotic paradox in, 169
neuroticism in, 154-55, 161
oral optimism in, 159
oral pessimism in, 159
organization of, 151-56
perception and, 166-68, 170
perceptual constancy and, 170
prefrontal lobotomy on, 166, 171
prejudice and, 193-95
psychotherapy on, 328
ratings of, preinformation on, 289
Rorschach test of, 154-55, 156, 163-64
self-system, 423
semistarvation on, 270
servo mechanisms and, 435
somatotyping, 155-56
Soviet writings on, 171
stratification of, 170
student, instructor's ratings of, 388-89
taxonomic variables in, 154
temperament, 141
tests, behavior abnormalities and, 386-87
theories of, 166-71
twin studies in, 160-61
type versus trait in, 152
see also Individual differences; and Intelligence
Physiological psychology, 233-56
autonomic activity, cortical influence on, 251-52
gastric motility, 252
hypothalamic parasympathetic centers, 252
integrative mechanisms, 254-56
mass facilitation, 255
mental functions, hemispherectomy on, 246
motor functions, 245-46
central mechanisms of, 245-46
cerebral, 245-46
disorders of, 246
hemispherectomy on, 246
motor cortex, representation in, 245
pyramidal tract and, 246
strength index of, 388
nervous structure and, 246-51
cortical areas, 246-47
extinction and, 249-50
sensory processes and, 246-51
orbital regions, 252
perceptual functions, 249-50
cerebral organizations of, 250
theory of, 250-51
sensory processes, 246-51
see also Animal psychology; Comparative psychology; Hearing; Olfaction; Somesthesis; Taste; and Vision
Prefrontal lobotomy, see Psychosurgery
Prejudice
authoritarian practices and, 194
changing attitudes, 195-96
in children, 13
family influence on, 14-

15
ethnic, 191-96
ethnocentrism; studies of, 193-95
group therapy on, 195-96
individual differences, 191-92
national stereotypes and, 192-93
personal contact on, 193
personality and, 193-95
reduction in, 195-96
sociocultural factors in, 191-92, 195
Psychiatry
ego strength in, concept of, 284-85
emotion-blood relationships, study in, 276-77
environment-heredity controversy and, 157
oral drive and, 18-19, 159
psychosurgery and, 262
psychotics, therapy on, 332
training in, 331-32
see also Psychoanalysis; and Psychotherapy
Psychoanalysis, 340-44
chair versus couch in, 341
children and, 342-43
classical analysis, 343
"corrective emotional experience" in, 343
counseling and, 351
failures, causes of, 343-44
Gestalt personality theory and 289-90
group therapy and, 335
integration concept in, 290
interpersonal relationships in, reality of, 341
interpretation in, new aspects of, 343
Neo-Freudian revolts in, 341
Oedipus complex and, 341
order in, 322
personality development, assumptions in, 190-91
and prejudice, interpretation of, 195
psychotics and, 342
therapist-patient relationships in, 342
transference reactions in, 343
see also Psychiatry; and Psychotherapy
Psychodiagnostics, 283-314
"Blacky Test", 290
color reactions in, 292
Comprehension-Vocabulary Similarities scale, 292
diagnostic groups, differentiation of, 301-2, 310-11, 312
diagnostic instruments, inviolability of, 291-92
drive theory and, 290
ego strength measurement, 284-85
experimental approaches to, 292
experimental sets, 288-89
on selective perception, 288-89
factor analysis in, 294-95
future of, 313
goals of, 284-87, 313
interviewing in, 285-86
mental-life levels and, 290
needs of, 313-14
overt behavior predictions, 285-86
perceptual hypothesis, 287-89
contemporary research and, 287-88
planned research programs in, 314
problem of, 284-90
profile analysis in, 293-94
see also Minnesota Multiphasic Personality Inventory
projective technique in, 286-87, 295-307
drive theory and, 290
rationale of, 286-87, 296
theoretical system for, 289
validating, 286
psychiatric categories in, 284-85
psychoanalytical theory in, 290
Gestalt personality theory and, 289-90
P-technique in, 295
research methodology, 291-95
scaling procedures in, 293
semantic misunderstandings in, 284
separate-category approach to, 302
subtest analysis and, 309-11
theoretical orientation of, 287-90
therapy predictions in, 286
trends in, 283-84
variables in, 286
see also Diagnosis; and Tests
Psychosomatics, 271-77
cardiovascular complaints and, 276
circulatory system and, 274-75
constipation and, 274
culture mores on, 275-76
deviations in, 272-73
diabetes and, 275
ejection-riddance reaction in, 274
emotional factors in, 273-74
gastric dysfunction and, 273-74
hippuric acid excretion and, 277
intrapsychic communication in, 277
migraine and, 272
organ functioning and, 273, 274
predisposing factor in, 275
protective reaction patterns in, 271-72
reflex sympathetic dystrophies and, 277
stress in, 244-45, 271-77
specificity of, 276
syndrome, 312-13
threat-response hypothesis, 272
unconscious conflicts on, 273
urticaria and, 276
Psychosurgery, 261-66
abstract-concrete attitude and, 264-65
anxiety, effect on, 262-63
cytoarchitectonics in, 263
defects from, 265-66
hippuric acid and, 277
intellect and, 264, 265
leucotomy, 253-54
mechanism of improvement from, 262
pain and, 106-7
planning ability and, 264-65
pneumoencephalographic studies after, 254
prolonged attention, 264-65
temporal lobectomy, 266
thalamatomy, 266
transient changes after, 263-64
types of, 263
on vision, 249
Psychotherapy, 321-48
adjuncts to, 346-47
adjustment level, ratings of, 327
approaches to, 340, 356
assessment of, 329-30
carbon dioxide inhalations in, 347-48
chronic patients and, 348
client's frame of reference in, 356
communication in, 326
consulting therapists in, 347
continuum notion in, 328
directive, 339-40
drive-reinforcement and, 345-46
drugs in, 348

"egg shell" attitude in, 323-25
electric shock in, 271
extrasensory perception in, 332
follow-up in, 321-22, 325-26, 339, 364-65
free association approach to, 355
Freudian, revolt against, 323
hypnodrama in, 347
hypnosis in, 346-47
individual and social adjustment and, 358
interpersonal relationships, changes in, 332
interpretation in, 356
learning theory and, 322-23, 344-46
methods in, 355-56
"multiple therapy" in, 324, 331
Neo-Freudians, approach of, 338
neurotic anxiety and, 436
nondirective, 337-39
client's frame of reference, 337-38
counseling and, 351
workbook in, 338-39
objective research, 323-30
barriers to, 323-24
devices in, 323-24
on schizophrenics, 325-26
therapists, attitudes and actions of, 326-27
outcome in, predictions of, 286
personality changes, predictions of, 328
principles of, 333
psychotics, 332
repressed material, release of, 332
schools of thought in, 332
self-evaluation in, 327-28
social implications of, 357 357-58
status of, 321-23
strength-threat in, 327
stuttering and, 346
Thematic Apperception Test in, 302-4
therapist as teacher in, 355-56
therapist differences in, 326-27
training in, 330-32
of chemical psychologists, 330
insight therapy, 331-32
recording devices in, 331-32
specific methods of, 330-31
treatability of patients, 286
variable studies in, 328-29
vitality of, 321
see also Counseling; Group Group psychotherapy; and Psychoanalysis
P-technique, see Factor analysis
Punishment
on behavior of children, 20
effects of, 425-26
on habits, 421
inhibitory dynamics and, 427-28
inhibitory effect of, 428
on learning, 420
response inhibition by, 421
status of, 424-28
stimulus-response sequence and, 420-21

Q

Q-technique, personality measurement by, 165-66

R

Reading, 401-4
ability of children, 398
determinants of success in, 402
disabilities
auditory defects and, 402-3
speech difficulties and, 402
failures in, 401-2
fixation pause duration in, 404
improvement of, 403
mental age and, 398
readability, formulas, 212-13
remedial help in, 404
sex on, 401
vocabulary and, 403-4
see also Education; and Learning
Reasoning, see Cognition; Learning; and Perception
Reinforcement
acquisition and, 35-36
on behavior of children, 8-9
drive identification and, 345-46
gradient of, 428
latent learning and, 31
learning and, 34-40
partial
extinction and, 36-37
learning under, 36-37
satisfaction and, 36-37
secondary, 37-38
appetite drives and, 428-32
definitions of, 429
sign learning and, 429
sexual stimulation in, 39
in stuttering, 346
tertiary motivation in, 29
Research design
latin square in, 415
statistical methods and, 409-17
trends in, 291-95
and variance technique, analysis of, 415-17
Retention
convulsive shock on, 254
cortical locus for, 255
frustration on, 10
of motor skills, 46-47
shock therapy on, 271
success on, 10
Rorschach test, 154-55, 156, 163-64, 287-88, 296, 297-302, 369-71
Army General Classification Test and, 299
as clinical instrument, 369
color on responses to, 300-1, 370
content analysis in, 297
criticism of, 369
diagnostic differentiation by, 301-2
diagnostic validity of, 301
experimental set on, 288-89
experimenter-differences on, 288
factor analysis of, 294
form-level appraisal and, 299-300
on homosexuals, 370-71
of individual differences, 147
inspection technique for, 297
of integrative capacity, 290
job satisfaction and, 222
limitations of, 371
normative data from, 297-300
personality change and, 329
in promotional-potentials determination, 212
protective service employees and, 213
in psychodiagnostics, 287-88
selected aspects of

scoring, 299
separate-category diagnosis and, 302
stimulus-properties of, 300-1
of superior adults, 299
threat and, 302
unpleasant concepts and, 307
validation of, 369-71
Wechsler-Bellevue Scale and, 369-70

S

Sensory processess, 246-51
accessory cells in, 247
allesthesia and, 249-50
central pathways of, 247-48
cerebral organization of, 246-47
discrimination in, 248-49
errors of localization and, 249-50
extinction and, 249-50
nervous structure and, 246-51
polysensory areas in, 247
semistarvation on, 270-71
somato-sensory receiving areas in, 247
spatial orientations in, 266
thresholds in, 248
visual areas in, 247
Servo mechanisms, 434-35
Social psychology, 175-204
boundaries of, 175-76
cognition of social facts, 178-79
culture and, 189-91
ethnic prejudice and, 191-96
group processes and, 175-201
in industry, 222-26
intergroup tension and, 191-96
perceptual accentuation hypothesis in, 177
prejudice, 191-96
reference group theory, 188-89
roles, 188
social class, 189
sociological concepts in, 187-88
see also Attitudes; Behavior, social; Group dynamics; Opinion methodology; and Perception
Somatic androgyny
measures of, 383
variables in, 5
Somesthesis, 105-19
cortical ablation on, 249
cortical sensory areas, 109-10
receptor localization in, 109-10
secondary, 110
cutaneous senses, 109-10
genital stimulation and, 109
muscle receptors and, 110-11
autogenetic inhibition and, 111
central mechanisms of, 111
cortical activity and, 111
peripheral mechanisms of, 110-11
temperature sensations, 108-9
chemical environment on, 109
cortical representation of, 109
physiology of, 108-9
thresholds of, 109
see also Olfaction; Pain; Taste; and Vestibular functions
Speech
development of, 1
difficulties of, 402
hearing of, 97
stuttering in, 346
Statistical methods, 409-17
attributes, 414
chi square, 412, 414
classification, 413-14
diagnostics and, 413-14
differential prediction in, 413
discriminant function, 146-47, 413
factor analysis, 412-13
item selection, rationale for, 410
latent structure theory and, 411
measurement error, 409, 410
concept of, 409
test scores and, 410
multiple-choice tests
guessing correction in, 412
maximum-likelihood estimate in, 412
multiple discriminant function, 146-47, 413
multivariate selection, 409
prediction, 413-14
psychometrics and, 409-12
reliability, 409, 410
research design and, 409-17
scale analysis and, 410-12
validity, 409, 410
variance, 414-17
Stimulus-response sequence, 236, 419-23
discrimination learning and, 29
drive increment on, 422
drive-reduction hypothesis and, 429-30
emotions on, 435
peripheral mechanisms of, 243-45
punishment on, 420-21
Rorschach test and, 300-1
sign learning and, 423
solution learning and, 423
stimulus-stimulus theory and, 31
Thorndike's Law of Effect, 420, 421-22
Stress, 266-67, 271-77
see also Psychosomatics
Strong Vocational Interest Blank, 209, 222, 372
Stuttering, 346
Szondi test, 296, 304-5, 372
associative valence in, 304
genotypic reliability of, 305
rationale of, 304-5
theoretical expectations from, 305
validity of, 372

T

Taste, 119-21
anatomical studies of, 119-21
blindness, 120
chemical studies of, 119-21
chorda tympani responses in, 120-21
differential thresholds in, 120
"duo-trio" test of, 120
enzyme theory of, 123
food preference in, 121
initial preference in, 121
monosodiumglutamate on, 119
physiological studies of, 119-21
race on, 120
rejection thresholds in, 120-21
sex on, 120
triangle test of, 119, 120
see also Chemical senses; and Olfaction
Tests
clerical, 213
construction of, 142-45

XUM

cross-validation of, 144
differential prediction from, 145-46
employment, 134
foreign service, 212
forced choice, 146
group oral performance, 210
homogeneity, 135-36
intelligence, 307-8
item analysis in, 142-43
mechanical aptitude, 208-9
multiple cut-off, 147
multiple regression in, 145-46, 147
nursing school, 212
objective, 307-13
Comprehension-Vocabulary-Similarities scale, 311
deterioration measures, 311-12
effectiveness of, 367-68
intelligence, 307-8
personality, 312-13
oral trades, 214
personality, 312-13
predictive validities of, 133-34
profile interpretations, 146-47
projective, 210-11, 295-307, 369-74
attitudes, 200-1
Basic Rorschach Score, 290
Children's Apperception Test, 307
in counseling, 369-74
figure drawing, 296, 306
finger painting, 296
in industry, 210-11, 296
Make-a-Picture Story, 307
Michigan Picture Test, 306
Mosaic, 296, 307
new, 372-73
in nonclinical areas, 296
Rosenzweig Picture Frustration Test, 12, 17, 200, 306, 389
sentence-completion, 296, 307
on social issues, 296
Symonds Picture-Story Test, 306-7
Vocational Apperception Test, 372
word association, 307
reliability, 143
scoring formulas for, 143
statistics of, 142
supervisory, 211-13
utilization of, 145-47
validity of, 137, 143-44
veterinary medicine aptitude, 211-12
see also Bender Visual Motor Gestalt Test; Factor analysis; Kuder Preference Record; Minnesota Multiphastic Personality Inventory; Psychodiagnostics; Rorschach test; Statistical methods; Strong Vocational Interest Blank; Szondi test; Thematic Apperception Test; Wechsler Bellevue Scale; and Wechsler Intelligence Scale for Children
Thematic Apperception Test, 296, 302-4, 371-72
alterations in, 303-4
behavior predictions and, 285
maturational processes and, 372
Negroes and, 372
normative data from, 303
overt behavior predictions and, 285
play therapy and, 303
stimuli values of, 371
validation of, 371-72
war themes in, 389
Transposition, see Learning

V

Variance
analysis of, 415-17
variance model in, components of, 416
heterogeneity of, 414-15
Vestibular functions, 112-18
action potential studies of, 113-14, 115-16
anatomical studies of, 112
canals and, mechanical principles of, 114
central mechanisms of, 115-16
chemical techniques on, 115
congenital deafness on, 115
drugs on, 112, 118
eye movements and, 116-17
recording of, 114
motion sickness and, 112, 117
drugs on, 117
otoliths and, 118
prevention of, 118
vomiting center in, 117
nystagmus and
drugs on, 118
induced, 115
plane of, 116
rotation on, 119
velocity on, 114
otolith displacement and, 115
peripheral mechanisms of, 112-15
rotation on, 118-19
threshold studies of, 112-13
vision and, 116-17
see also Ear
Vision, 55-80
achromaticity and, 60
basic processes in, 62-70
blinking in, 77
brightness in
area on, 61
shape on, 61
chemistry of, 66
color
discrimination in, 67-68
heredity on, 56
ICI system and, 59, 69
individual differences in, 56
measurement of, 56
perceptual modes in, 60
pigment mixtures in, 58
polychromatic theory of, 70
stimuli in, 59
temperatures in, 58
trichromatic theory of, 69-70
color television on, 67
colorimetry in, field conditions on, 68-69
comfort in, 77-79
critical fusion frequency in, 67
dark adaptation in, 62-65
measurements of, 62-63
mechanisms of, 62
pre-adapting light on, 63-64
red adaptation on, 62-63
retina in, 64
vitamins on, 64
daylight on, 77-78
dimensional processes in, 58-61
dimensions of, 59-60
drugs on, 64
efficiency in, 77-79
electrical stimulation of, 70
episcotiser and, 57
esthetics in, 80
experiments in
apparatus for, 57-58
standardization of, 59
flicker tolerance, 67
fluctuations in
eye movements on, 64
thresholds on, 64
fluorescent lights on, 77-78, 79
hemidecortication on, 249

mechanisms of, 62-70
muscular tension in, 77
night, myopia in, 64-65
obstacle perception and, auditory cues in, 80
optics of, 56
phylogenetic development of, 55
physical space on, 74-75
psychological factors on, 227-28
psychological scales in, 61-62
psychophysics in, 61-62
methods in, 61-62
multidimensional, 60-61
quantum theories of, 65-66
retina in
brightness contrast in, 250
mechanisms of, 57-58
semistarvation on, 270-71
sex on, 80
stimulus correlates in, 58-59
thinking in, primary abilities in, 80
threshold data in, 61-62
vestibular control and, 116-17
visual space on, 74-75
see also Color blindness; Sensory processes; Vestibular functions; and Visual perception
Visual perception
acuity in, 70-72
cones on, 72
light on, 77-78
physiological nystagmus on, 72
cortical ablations on, 249
equilibration and, 70-80
labyrinthectomy on, 79-80
sex on, 79
electrical stimulation on, 73-74
figural aftereffects in, 72-76
retinal correlates for, 73-74
satiation on, 72-73
goodness of form, 75-76
legibility in, 70-72
movement, 77
size constancy, 76-77
age of appearance, 76
spatial discrimination in, 56, 70-77
brightness on, 71-72
electrotonus on, 74
light and shadow in, 76
mathematical theory of, 74-75
movement on, 77
size constancy on, 76-77
vernier and real depth on, 71-72
see also Vision
Vocational counseling
Vocational Apperception Test, 372
see also Counseling; Industrial psychology; and Tests

W

Wechsler-Bellevue Scale, 308-12, 375
administration of, 308
as deterioration measurement, 311-12
general properties of, 308
reliability of, 308
Rorschach and, 369-70
scoring of, 308
short forms of, 308-9
subtest analysis, diagnosis by, 309-11
Wechsler Intelligence Scale for Children, 384, 398-99

XUM